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Edited for the Council by J. LAURENCE PRITCHARD, Hon. Fellow

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NOTICES

International Air Congress, Rome, 1927

The opening date of the International Air Congress which is to take place in Rome has been postponed to October 24th. Further particulars may be obtained from the Secretary General, Major Pier Francesco Bitossi, IV. International Air Congress, Via della Mercede, 9, Rome.

Lecture Programme, Autumn, 1927

The following is the first half of the programme of lectures for next Session, so far as at present arranged. All the lectures will be held in the Theatre of the Royal Society of Arts, 18, John Street, Adelphi, W.C.2, at 6.30 p.m., except the Joint Meeting with the Institution of Automobile Engineers, which will take place at 7.45 p.m.

Oct. 6.—Mr. F. Handley Page, C.B.E., F.R.Ae.S. (Title to be announced later.)

Oct. 13.—Squadron Leader T. H. England, D.Sc., A.F.C., "The Practical Side of Performance Testing of Aircraft."

Oct. 20.—Mr. M. L. Bramson, A.C.G.I., A.F.R.Ae.S., "Safety Devices for Aircraft."

Nov. 3.—Joint Meeting with the Institution of Automobile Engineers. Mr. H. B. Taylor, A.F.R.Ae.S., "High Speed Compression Ignition Engine Research."

Nov. 17.—Mr. R. K. Pierson, A.M.Inst.C.E., F.R.Ae.S., "The Use of the Wind Tunnel in the Prediction of Aeroplane Performance."

Nov. 24.—Major C. J. Stewart, O.B.E., M.I.Mech.E., F.R.Ae.S., M.I.A.E., "Modern Developments in Aircraft Instruments."

Dec. 1.—Major J. D. Rennie, A.R.T.C., A.M.Inst.C.E., F.R.Ae.S., "The Problem of the Long Range Flying Boat."

Dec. 8.—Captain F. Entwistle, B.Sc., "Fog."

Dec. 15.—Major F. A. Bumpus, "The Development of Torpedo-Carrying Aircraft."

Advance Proofs of Lectures

Advance proofs of the papers to be read in the Autumn programme (price 6d. each, or 4/6 for the series) will be sent on request to those wishing to read the papers beforehand, as soon as they are available from the printers.

List of Members

A list of members has been printed, and is circulated with this number of the JOURNAL. The list is available, free, to members of the Society only.

Associate Fellowship Examination

Part I. of the Associate Fellowship Examination will be held on Tuesday, September 20th, in the Society's offices.

Library

The following books and pamphlets have been received recently and placed in the Library :—

Reports and Memoranda of the Aeronautical Research Committee—

No. 1001. "The Spinning of Aeroplanes," by S. B. Gates and L. W. Bryant.

No. 1053. "Torsional Vibration in Engines," by B. C. Carter.

No. 1065. "Preliminary Experiments on Two-Dimensional Flow round Bodies moving through a Stationary Fluid," by B. M. Jones, W. S. Farren and C. E. W. Lockyer.

No. 1066. "Wind Tunnel Experiments on a Symmetrical Aerofoil (Göttingen 429 Section)," by C. N. H. Lock, H. C. H. Townend and A. G. Gadd.

No. 1076. "Comparison of Atalanta and Model Seaworthiness and Fore and Aft Angle," by the Staffs of M.A.E.E. and Wm. Froude National Tank, N.P.L.

No. 1077. "Lateral Stability with Special Reference to Controlled Motion," by H. M. Garner.

No. 1079. "Summary by the Secretary Engine Sub-Committee of 'a Report on Anti-Knock Investigations,'" by A. Egerton and S. F. Gates.

No. 1080. "Note on the Reduction of Performance to the Standard Atmosphere," by R. A. Capon.

No. 1082. "Pressures Round a Cylinder Rotating in an Air Current," by A. Thom.

No. 1084. "Paradox in Fluid Motion," by H. Lamb.

Reports of the National Advisory Committee for Aeronautics of the United States—

No. 257. "Pressure Distribution over a Wing and Tail Rib of a Ve-7 and of a TS Airplane in Flight," by J. W. Crowley.

No. 258. "Some Factors Affecting the Reproducibility of Penetration and the Cut-off of Oil Sprays for Fuel Injection Engines," by E. G. Beardsley.

No. 259. "Characteristics of Propeller Sections Tested in the Variable Density Wind Tunnel," by E. N. Jacobs.

No. 260. "Effect of a Flap and Ailerons on the N.A.C.A.-M6 Airfoil Section," by G. J. Higgins and E. N. Jacobs.

No. 261. "Resistance and Cooling Power of Various Radiators," by R. H. Smith.

No. 265. "A Full-Scale Investigation of Ground Effect," by E. G. Reid.

Memoirs of the Royal Meteorological Society, Vol. I., Nos. 4, 5, 7 and 8.

Geophysical Memoirs of the Meteorological Office—

No. 31. "Classification of Monthly Charts of Pressure Anomaly over the Northern Hemisphere," by C. E. P. Brooks and W. A. Quennell.

No. 35. "A Comparison of the Records from British Magnetic Stations Underground and Surface," by C. Chree and R. E. Watson.

Reports of the Meetings in Zürich, 1926, of the Commissions for Terrestrial Magnetism and Atmospheric Electricity and for the Réseau Mondial, also of the Commission for Synoptic Weather Information.

Miscellaneous Publications—

"The Story of the Airship," published by the Goodyear Tyre and Rubber Co.
"Basic Principles of Air Warfare," by "Squadron Leader."
"Navigational Wireless," by S. H. Long.
"A Manual of Rigging," A.P.1107 Air Ministry.
"Air Force Act." (Revised to include all amendments made down to the date of the passing of the Army and Air Force (Annual) Act, 1927.)
"Vier Abhandlungen sur Hydrodynamik und Aerodynamik," by L. Prandtl and A. Betz.
"Schweizerisches Luftretcht," by F. Hess.

J. Laurence Pritchard, *Secretary*.

PROCEEDINGS

NINTH MEETING, SECOND HALF, 62ND SESSION

The Ninth Meeting of the Sixty-Second Session of the Royal Aeronautical Society was held in the Theatre of the Royal Society of Arts, 18, John Street, Adelphi, W.C.2, on Thursday, March 31st, 1927, when Dr. E. G. Richardson read his paper on "Recent Model Experiments in Aerodynamics." Colonel the Master of Sempill, Chairman of the Society, was in the chair.

RECENT MODEL EXPERIMENTS IN AERODYNAMICS

BY E. G. RICHARDSON, B.A., M.SC., PH.D.

There are certain problems in aeronautics which, as pointed out by Major Wimperis in a recent lecture to the Institute of Physics, are hardly engineering problems, nor are they entirely mathematical. In such problems you can choose your experimental conditions to a certain extent, but they are not arbitrary. They lie within the domain of physics, and among them the flow of a real fluid under various conditions has peculiar interest to the physicist.

The experiments I am about to describe to you were begun five years ago in the Physical and Aeronautical Departments of East London College under the guidance of Professor C. H. Lees and Dr. N. A. V. Piercy. My removal to University College shortly after, necessitated the temporary abandonment of a subject in which I had meanwhile become intensely interested, but Professor Porter kindly placed facilities at my disposal which permitted the erection of a small wind channel in one of the rooms of the Carey Foster laboratory, and that has been in constant use during the past two years. For part of this period I had the additional advantage of the valuable help of a colleague, Mr. Tyler, who carried out work on channel interference with vortex motion and on rotating cylinders at my suggestion. It will be understood that it was necessary to employ simple apparatus, not having the resources of a State or industrial laboratory at our disposal; but this in some ways was an advantage, as it kept one's attention directed on the "fundamentals" of the problem, leaving the "frills" for those better equipped.

Flow Past a Stationary Cylinder—The Boundary Layer

Consider a solid cylinder placed in a moving fluid. The ideal motion, the most amenable to mathematical treatment, is that in which the particles are supposed to exercise no dragging effect on each other. The paths of particles in an infinite channel in which uniform flow is taking place will then be represented by parallel straight lines—the so-called streamlines. On dipping the cylinder into such a fluid we introduce the "boundary condition" that at the surface between the water and the cylinder there can be no normal velocity. This leads to the requirement that the surface must itself be a streamline. The paths of particles which pass near the cylinder are shown in the figure.

The assumption of no friction in the fluid requires that the kinetic energy gained by particles passing from the front to the mid-point of the side of the cylinder shall be just lost when they reach the stern, so that they leave the cylinder with the same velocity with which they struck it. The fluid which actually

passes in contact with the body therefore slips past the surface without any retardation. This leads to the paradox that there is no force on the cylinder due to this co-called "potential streaming." This is in obvious contradiction to fact, and yet, except very close to the cylinder, these ideal streamlines do very closely resemble those observed experimentally by the method of Hele Shaw, provided that the velocity of flow is not too great. The mistake arises evidently in neglecting to take into account the friction, not only between the solid boundary and the adjacent fluid which prevents slip to the extent postulated, but also between neighbouring layers, which approximates the velocity, if any, at the boundary to that in the body of the fluid, so as to produce a steep velocity gradient across the streamlines near the surface of the body. There is thus a shearing force on the fluid tending to retard it; or, if we think of the contrary but dynamically identical case of the cylinder dragged through the fluid, there is a *resistance* offered by the cylinder as there would be if it were dragged over a

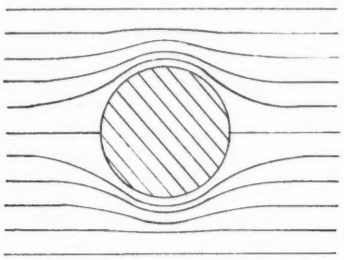

Fig. 1.

solid surface. The part of the resistance due to shearing effects between the body and neighbouring strata of fluid is known in this country as "skin friction," and must be distinguished from the resistance due to eddy friction in the rear of the obstacle which will be considered later. When the body is without sharp changes of contour and moves at a low velocity through the fluid, skin friction produces the major part of the drag resistance.

A large number of attempts have been made to introduce viscosity terms into the mathematical equations of fluid motion, but an exact solution has so far defied all efforts. An idea developed by Prandtl,[1]* though admittedly an approximation, has stood the test of twenty years' application to the prediction of skin resistance of profiles.

Prandtl starts with the assumption that the viscosity of air being a small quantity, its effects can be neglected except in the neighbourhood of a solid boundary, where the condition that there shall be no slip of the stream past the boundary requires a very steep gradient of the tangential velocity from its mean value in the main stream to zero at the boundary. The operation of viscosity is restricted by Prandtl to a thin "boundary layer," outside of which potential streaming takes place and inside of which the motion is also in general steady but with rapidly diminishing velocity across the layer. In order to calculate the thickness of this layer, Prandtl takes a new co-ordinate η normal to the surface, such that $\eta=0$ at the surface and $\eta=\infty$ at the outer edge of the boundary layer. He writes down the equations of motion introducing the viscous term $\mu_1 \delta V_x/\delta\eta$ representing the shearing force between adjacent strata of the layer where μ_1 is an enhanced coefficient of viscosity consequent on the reduced scale of η. In the steady state wherein the velocity at any point is constant with regard

* See bibliography at end of this paper.

to time, it appears that the thickness of the boundary layer δ depends on $\sqrt{(\mu l/\rho V)}$ where V is the average velocity outside the layer, ρ is the density and l is the length in the direction of motion of the surface on which the fluid has rubbed. The layer accumulates with l, and has been estimated at 1 or 2 cm. thick at the end of a plane wooden boundary, 2 metres long. If the quantity $V\delta/\nu$ (Reynolds' number) exceeds a critical value, due to increase of V or of δ, or of both, the motion in the boundary layer becomes turbulent. At this stage the motion is very unstable and the fluid in the layer tends to secede from the boundary and become involved with the liquid in the main stream.

When the velocity in the main stream is not steady but varies with time in an uncertain way (turbulence) the equations of Prandtl defy solution. When V varies with time, but in one special fashion, *i.e.*, in a simple harmonic way, it is however possible to solve them. It is evident that δ will then depend on the frequency for the maximum velocity in a simple harmonic motion is $2\pi n a$

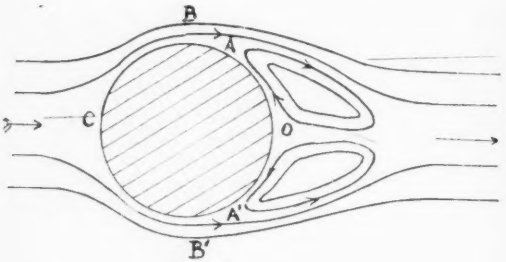

Fig. 2a.

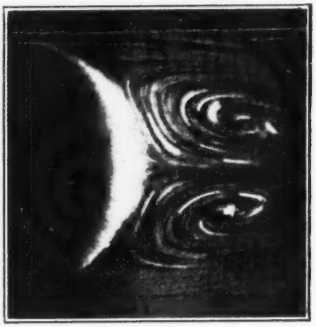

FIG. 2B.

where a is the amplitude. This case corresponds to the propagation of waves along a boundary, and the friction in the boundary layer will cause diminution in the amplitude and the velocity of propagation of the waves along the boundary. This has been studied experimentally by the speaker, using narrow rubber tubing in which the boundary layer might be expected to occupy the whole cross section—or, in other words, the effect of viscosity manifest over all the air in the tubing, as in the general theory of capillary flow—with a loose-fitting piston at one end producing simple harmonic displacement of the air. Using a hot wire anemometer the amplitudes at different lengths from the source of the disturbance, allowing for reflection, were made and a value $\nu_1 = 1.3$ found, on comparing results with the theory, in place of the usual .15 for air.[2] This value of ν, however, will be a function of the roughness and elasticity of the tube. Both these were considerable in the material chosen, and the experiments *inter alia* illustrated this point in Prandtl's theory.

Even with the restriction that in a thin layer close to the surface of the body immersed in a fluid the tangential velocity falls from the mean velocity of the fluid to zero, the ideal potential streaming of classical hydrodynamics is not fulfilled in real cases save at very slow speeds. The fluid passing a cylinder such as that depicted in Fig. 2, does not hug the stern, but leaves it at the " secession points " A and A'. Prandtl explains this as follows:—In passing from C to B and B' the fluid in the boundary layer gains kinetic energy as it is moving faster at B and B' than at C; also the pressure at C is greater than at B and B'; passing from B or B' to O the fluid loses this kinetic energy as it is passing

into regions of increasing pressure. In the absence of friction the fluid in the layer would be able to reach O, and resume its original velocity if the pressure at C equals the pressure at O, but owing to friction it is brought to rest at A and A' short of O, as a car on a switchback fails to reach its former height, owing to energy losses by friction.

A region of dead water is accordingly formed behind the obstacle and owing to the shearing effect of the swiftly flowing main stream on this region, the fluid in it is set in rotation in the form of two eddies revolving in the sense shown (Fig. 2). These stationary eddies have been photographed by Nisi and Porter,[3] one of their photographs being shown in Fig. 2B. When the velocity in the main stream is sufficiently great the eddies are carried away with the stream.

These vortices represent energy carried away from the body to be dissipated in friction as heat, and so have an importance in the estimation of the resistance opposed by the body to the stream, in comparison with which the friction in the boundary layer is small. If by some means the loss of kinetic energy by the fluid in the boundary layer be reduced, *e.g.*, by moving the whole surface of the cylinder in the direction CBO, or by blowing air out of the hollow interior at a point between C and B, or finally by sucking air in between B and O, V may be increased to higher values without the formation and loss of eddies at the stern and with consequent reduction of the drag resistance.[4]

The procession of vortices behind a cylinder past which water was streaming was first noted by Mallock,[5] and systematically photographed by Bénard.[6] In general, the vortices are detached from opposite sides of the cylinder alternately and form an avenue of alternate vortices with constant separation l between two in one row, and distance h between the rows. Kármán[7] in a notable paper showed that such a system was more stable than one in which the vortices in the avenue were opposite, and further, that maximum stability occurs when $h/l=0.28$, though Levy[8] has subsequently shown that no such system is completely stable. The periodic detachment of these eddies causes a periodic alternating cross force on the body, tending to make it vibrate across the stream. If the body is a wire tuned to the frequency of the detachment of the vortices, the wire "sings" this tone. These æolian tones were first measured by Strouhal in 1878; he also established the fundamental formula connecting the width of the wire (=diameter of the cylinder D) with the frequency n of the tone and the velocity V of the stream, *i.e.*, V/nD is constant.

Some later work on the lines of Bénard was performed by Kármán and Rubach,[9] who found from their photographs that h/l was a constant, independent of D and of V. Also h was approximately proportional to and greater than D, so that $h=bD$, say; the velocity U of the vortex system relative to the (stationary) fluid was less than, but proportional to, V, *i.e.*, $U=aV$. These facts led Krüger and Lauth[10] to a theoretical basis for Strouhal's formula. The frequency of the vibration represents the number of vortices formed on one side of the wire in one second. If the body swings with the vortices $l/(V-U)$ is the time between the disengagement of two successive vortices from the same side of the body, *i.e.*, the period of swing:—

$$1/n=l/(V-U)$$

$$V/nD=(V/D)\{l/(V-U)\}=(l/D)\{V/(V-U)\}=b/(1-a) \quad . \quad (1)$$

which is a constant. From Kármán and Rubach's results for a and b Krüger and Lauth obtained the value 5.0 for this constant, which is in fair agreement with Strouhal's numbers, which lie between 6.3 and 4.9. Krüger and Lauth were thus the first to demonstrate the fact that the tones were due to the detachment of the vortices and had in fact the same frequency.

Riabouchinsky found that a pendulum would swing across an air current if the period coincided with that of the production of pairs of vortices.

In the work which I describe below I have endeavoured to evaluate this constant for as large a range of diameters as possible, and by as many methods as occurred to me. It is impossible to cover the whole range by one method, as thin wires require a different form of constraint from thick wires, or from rods, in order to get a good observable vibration.

Brass hollow rods ranging from 1 to 5 cm. in diameter were suspended in pendulum fashion so that their lower ends dipped into water contained in a circular tank which could be rotated at constant speed by an electric motor. The general velocity of the water V was measured by timing little pieces of paper on its surface when resonant vibration was produced in the pendulum; n was determined by counting swings, and could be altered by cutting pieces off the rod. The results are shown in Table 1.

TABLE 1.

D	n	V	V/nD	D	n	V	V/nD
0.95	1.54	8.7	5.95	2.54	1.33	18.5	5.5
0.96	1.54	8.9	6.1	2.80	1.25	20.5	5.85
1.30	2.50	20.0	6.15	2.80	1.05	13.8	4.7
1.30	1.29	9.5	5.7	2.80	0.91	15.0	5.9
1.30	2.10	15.0	5.5	3.85	1.33	22.5	4.4
1.60	2.20	18.5	5.25	3.85	1.05	22.2	5.5
1.90	1.22	12.5	5.4	4.10	0.90	19.4	5.3
1.90	1.25	12.5	5.25	4.10	1.33	24.0	4.4
1.90	1.82	19.5	5.65	4.80	1.57	28.6	3.8
—	—	—	—	4.80	1.53	24.0	3.3

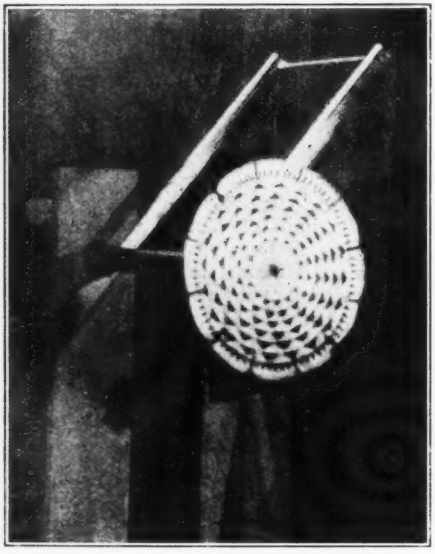

FIG. 3.

The first method I used for the investigation of the æolian tones of *wires* was Strouhal's. The "whirler" rotated about a horizontal axis to which two stout steel bars were rigidly attached, each having a little clamp at the further

end to hold the wire and adjustable counterpoise weights on the side of the axis opposite to the wire. The little clamps were passed through holes in the steel bars, in which they were held by thumbscrews by means of which the tension on the wire and thereupon its fundamental frequency, could be altered. Three pulleys of different sizes were attached to the axle, also a stroboscopic disc, for estimating the speed of rotation. The wire was 45·7 cm. from the axis of rotation (nearly double that in Strouhal's apparatus), and as the axle was only 4 cm. thick, it was hoped that there would be little viscous drag of the air, especially as the long radius reduces the angular velocity necessary. The apparatus (shown in Fig. 3) was driven by a long belt from a motor in an annexe so that the noise of the motor was scarcely audible. With the radius stated, V was found by multiplying the number of revolutions per second by 286. The frequency of the note was found (after Strouhal) by tuning a sonometer to unison with it. Besides the fundamental tone of the wire, " harmonics " in which the wire vibrated in segments could be heard by suitably increasing the speed.

TABLE 2.

D	n		V	V/nD	D	n		V	V/nD
	Theor.	Obs.				Theor.	Obs.		
0.025	1,536	1,530	405	10.5	0.03	2,175	2,175	634	9.7
0.025	2,048	2,020	465	9.2	0.03	3,625	—	950	8.7
0.025	3,560	3,600	633	9·75	0.03	2,048	1,960	486	8.3
0.025	1,460	1,460	405	11.1	0.03	2,560	2,660	705	8.85
0.025	2,280	2,210	480	8.65	0.03	3,072	3,160	755	7·95
0.025	2,920	2,930	558	7.6	0.05	720	720	358	9·95
0.025	3,650	3,500	790	9.1	0.05	1,240	1,230	538	8.65
0.025	1,660	1,720	353	8.2	0.05	677	677	356	10.5
0.025	2,075	1,960	390	7·5	0.06	1,536	1,520	476	5.2
0.025	2,600	2,550	476	7·5	0.06	1,024	1,020	323	5·3
0.03	725	725	190	8·75	0.06	623	623	323	8.65
0.03	1,450	1,410	356	8.4	0.06	1,246	1,250	430	5·75

The notes that were heard plainly were always of high frequency and therefore difficult to estimate. A method was next devised, which should enable n and V to be determined more accurately, by removing the doubt whether the velocity measured was really that of the wire, relative to the surrounding air.

The idea of this method was to place a stationary wire in a current of air instead of having the wire moving through a (more or less) stationary fluid. By the kindness of Dr. Piercy I was enabled to set up my wire in a wind channel in the Aeronautical Laboratory at East London College. The channel in question had a long glass window in one side, facing a smaller window in the opposite side. The wire was so placed in the channel that it could be observed through a telescope near one end of the long window, in a direction inclined about 20 degrees to the axis, the wire being illuminated by an opal glass lantern placed behind the small window (Fig. 4).

The vibration of the wire was observed through the telescope of short focus, the frequency being found by a stroboscopic method. A motor carrying a stroboscopic disc having twelve slits equally spaced round its circumference revolved directly in front of the object glass of the telescope so as to cut off the light twelve times in each revolution. The speed of this motor was adjusted by means of a rheostat, until the fastest speed of the disc at which the vibrating wire appeared stationary was obtained. This speed in revolutions per second was then known by looking through a stroboscopic vibrator, just beside the telescope, and the speed multiplied by 12 gave the frequency of the wire indepen-

dently of the ear or of any comparison of pitch. The velocity of the air current was found from a xylol manometer which was calibrated from a pitot tube, placed at a point where the centre of the wire would come in the actual experiment. The wires were fixed vertically in small clamps, as before, with adjusting screws for altering the fundamental frequency. For diameters greater than $\frac{1}{2}$ mm. rubber cords of circular section were used; they were about 30 cm. long between the clamps, and each end was about 10 cm. from the wall of the channel (Table 3).

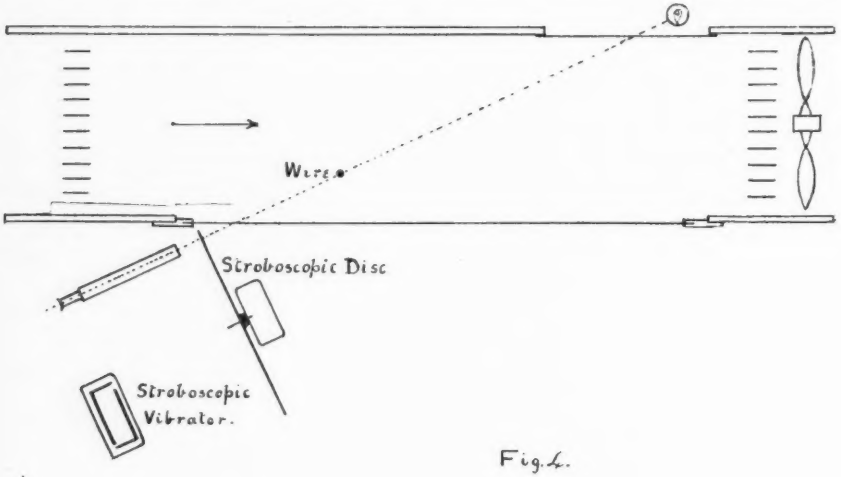

Fig. 4.

TABLE 3.

D	n Theor.	n Obs.	V	V/nD	D	n Theor.	n Obs.	V	V/nD
0.020	1,020	1,020	210	9.55	0.195	380	375	385	5.25
0.020	1,530	1,560	280	9.0	0.195	190	192	190	5.15
0.020	2,040	2,020	360	8.9	0.21	304	304	315	4.95
0.020	2,550	2,650	405	7.65	0.28	210	212	320	5.4
0.020	3,060	—	450	7.35	0.32	180	178	305	5.4
0.025	1,400	1,400	265	7.55	0.32	300	300	520	5.4
0.025	1,750	1,750	320	7.3	0.435	128	127	310	5.6
0.025	2,100	1,970	360	7.3	0.435	192	189	450	5.45
0.03	1,024	1,025	315	9.3	0.44	168	174	415	5.6
0.03	1,536	1,540	385	8.15	0.44	224	220	515	5.3
0.03	2,048	2,060	490	8.0	0.44	280	276	640	5.3
0.03	2,560	—	560	7.3	0.47	152	150	380	5.4
0.06	269	269	215	8.0	0.89	90	90	450	5.6
0.06	538	538	260	8.2	0.89	135	133	670	5.65
0.09	270	274	175	7.1	1.14	90	94	610	5.7
0.09	405	405	260	7.2	0.07	258	258	163	9.0
0.09	540	540	340	7.0	0.07	387	387	225	8.2

In the series of harmonics given out by a single wire, the velocities observed were not strictly in the ratio of the natural numbers, but were such as to give higher values for V/nD for the lowest note, or two lowest notes in the harmonic series, and this independently of the absolute value of the velocity. The very loose resonance shown by wires and cords excited in this manner, *i.e.*, by vortical

turbulence, especially when vibrating in several aliquot parts, constitutes an unavoidable difficulty in determining the value of the velocity which best excites a particular tone and renders it imperative to "strike a mean" in filling the third column.

The third series of experiments with wires consisted of an examination of their æolian vibrations in the revolving water tank, for comparison with the figures obtained from vibrations in an air current, given in the two preceding tables. The wires were held in a sort of hacksaw frame, with similar adjustable clamps. They were about 20 cm. long and about one-third of their length was submerged in order that a fixed microscope could be focussed on the mid-point of the wire and just clear the revolving tank. The frequency was found by the same stroboscopic method, save that the disc revolved in front of the lamp which shone on the wire, rendering the illumination, instead of the view, intermittent. Using a micrometer eyepiece, it is possible by this method to measure the amplitude as well as the frequency of a vibration, if both are required. The wires of diameter greater than 1 mm. were clamped at one end only in order to get a larger vibration. The frequency was then raised by cutting pieces off (Table 4).

TABLE 4.

D	n	V	V/nD	D	n	V	V/nD
0.020	240	41.0	8.55	0.142	40.0	40.0	7.05
0.020	300	52.0	8.65	0.142	33.3	34.5	7.3
0.020	355	58.5	8.25	0.200	47.0	63.0	6.7
0.030	144	37.5	8.65	0.200	45.8	60.0	6.55
0.030	216	52.0	8.05	0.200	26.0	31.0	6.0
0.030	282	68.5	8.05	0.205	28.4	41.0	7.0
0.050	133	57.3	8.6	0.233	31.6	44.0	6.0
0.050	230	88.0	7.65	0.233	42.0	60.0	6.1
0.050	212	80.5	7.6	0.233	45.0	68.5	6.5
0.075	133	77.0	7.7	0.300	14.0	26.0	6.2
0.075	148	86.5	7.8	0.300	20.0	40.0	6.65
0.075	160	84.5	7.05	0.320	21.0	39.5	5.9
0.090	118	83.0	8.55	0.320	35.3	61.5	5.5
0.090	120	84.5	7.85	0.475	20.0	50.0	5.55
0.090	160	112.5	7.75	0.475	25.0	72.0	6.6
0.090	212	135.0	7.1	0.475	37.5	100.0	5.6
0.142	37.5	38.5	7.2	0.960	10.0	49.5	5.5
0.142	30	32.0	7.5	0.960	14.0	73.0	5.4

The results of the three preceding tables are in substantial agreement with the more limited results of Rayleigh,[12] Krüger[13] (using the tank method) and Relf[14] (whirler method). They show a gradual rise in V/nD (Fig. 5) from 5 to 8 when the diameter is less than 3 mm., which is possibly due to mutual interference of the outer portions of alternate vortices. There were also small abnormalities in the values of the constant at high frequencies.

The rectitude of the applications of the law of dynamical similarity expressed in equation (1) has been called in question by Bénard himself in some recent papers,[15] in which he quotes my results as supporting his. It is true that I find a considerable variation of V/nD with D when D is small, indicating that the "constant" should rather be expressed in the form $V/n\,(D-D_0)$ where D_0 is a small constant, having the dimensions of a length. I do not agree, however, that the work of Kármán should be regarded as idealistic and that his conclusions are invalid where real fluids are concerned. On the one hand, the experiments involved are admittedly difficult; on the other hand, Kármán's calculations involve approximations to the real state. It is a matter of individual

taste whether the discrepancies are to be regarded as small blemishes on the theory or on our experimental technique, or as entirely invalidating its application. M. Bénard takes the latter view, but I prefer the former.

It remained to investigate the effect of viscosity on the motion. The quantity V/nD is of zero dimensions; if we are to introduce viscosity, it must enter in the form VD/ν, which is also of zero dimensions. Rayleigh put

$$V/nD = Kf(VD/\nu) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where K is a non-dimensional constant and ν is the kinematic coefficient of viscosity. In order to investigate this effect, I conducted observations on the wires already mentioned, in the tank filled with a mixture of water and molasses in varied proportion. In spite of the large variation in viscosity—.01 to .65—the results (shown by crosses) fall close to those of the air and water curves shown on Fig. 5.

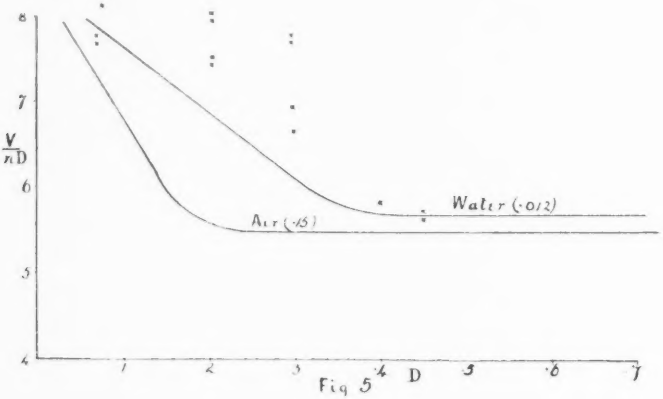

Fig 5

The form of the function $f(VD/\nu)$ could be found from results in liquids of different viscosity. Our results indicate that it is $(VD/\nu)^0$ or independent of viscosity over practically the whole range investigated, but that the periodic shedding of vortices to which n is due starts at a definite value of VD/ν.

The evidence for the last statement is as follows. With a string tuned to a definite pitch n, it is generally possible to get an æolian tone when V/nD is 5. As the tension in the string is released, n and V for this tone fall, until at a definite value of n no tone is produced at the appropriate value of V. The inference is that the alternate vortices have ceased to be shot off. The value of VD/ν when the tones failed to be heard were collected by the author and found to cluster round the value 30 for a wire in the molasses mixture. Examining the air results alone, one finds a slightly higher value for this minimum.

The principle of dynamical similarity teaches us in this case that for equal values of VD/ν the motion in the stream will be similar round two bodies of the same shape but of different size. The series of transformations will be identical, though in a different scale, but will take place at different rates. This is what is implied by the statement V/nD is constant. If then the flow is steady (in streamlines) round one wire at a given value of VD/ν, it will be steady round another wire of different D, but at the same VD/ν. If vortices begin to form behind one wire at a critical value of VD/ν then vorticity will appear behind the other wire at the same critical value. That, after the initiation of this oscillatory motion, the similarity does not seem to depend on VD/ν is a consequence of

the experimental fact that all values of VD/ν above the critical, the vortices form at the same rate, depending on V and D only.

To sum up, viscosity in the guise of the expression VD/ν determines when vortical motion shall commence, but once initiated, has no effect on the period.

Photographs of Vortices and Swinging Pendulum

I was anxious to obtain photographs of the vortices in a liquid behind a swinging pendulum, *to show their relation with the movements of the latter.* Photographs of these vortices have generally been made (following Bénard) with a fixed cylinder, the camera being pointed vertically upwards or downwards into the liquid; but this would not have shown the movements of the free cylinder. I arranged my camera with its axis behind the pendulum at about 30° to the surface of the water, and arranged a flashlight upstream which would project

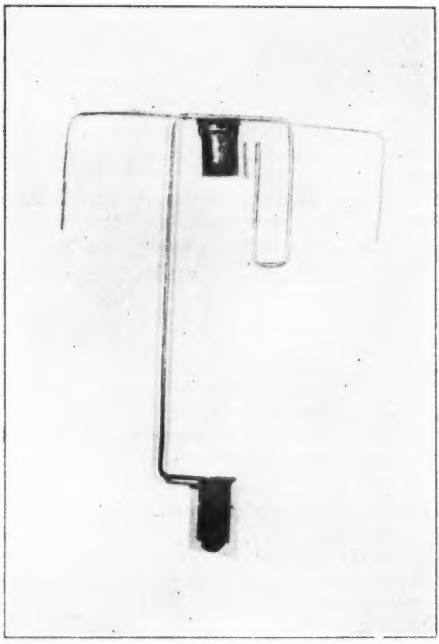

FIG. 6.

light on to the surface of the water in the tank and be reflected by that surface into the camera, so that the vortices would appear as dark dimples on the water. Brass and then glass pendulums were used at first, but both these spoilt the effect by their shadows on the water. Finally a skeleton pendulum (Fig. 6) was used, consisting of a short cylindrical "bob" with a wire support bent out of the field of view immediately above the level of the liquid. Photographs were obtained of various phases of the motion determined by the instant at which the "flash" was set off. This phase of the vibration at which the photograph was to be obtained could be controlled by the following device. A piece of brass wire was soldered across the top of the pendulum above the axis of rotation, and the ends of the wire were bent down at right angles so that one end could dip

into a small cup of mercury (this wire can be seen in the photograph). The flash was lighted by "blowing" a fuse placed just over it, by completing an electric circuit through the fuse, a cut-out switch, the brass peg on which the pendulum rotated and the mercury. The level of the mercury controlled the phase. For phases of the other half-period, the mercury cup was moved under the other wire tip. The action of taking a photograph was as follows:—The cap was taken off the camera in subdued light, with the cut-out switch open, the mercury was raised until the connecting wire just dipped into it at the required phase; when the pendulum had acquired a good vibration in the moving liquid in the tank, the switch was closed just before the wire dipped into the mercury. At the instant that contact was established up went the flash and the photograph was taken. In every case the liquid was flowing towards the camera. A number of successful photographs were taken showing the formation of the vortices and the vortices floating down stream.

Interference of Walls of Channel on Eddying Flow

Krüger and Marschner[16] had in 1922 examined the eddying motion produced behind a disc which nearly closed one end of a narrow circular section channel so as to leave an annular slit through which fluid was forced. It was thought by the writer, though the comparatively restricted results of Krüger and Marschner did not support the idea, that the flow outside would exhibit vortex rings behind the disc, corresponding to the vortex filaments behind a cylinder. As these would probably be modified by the proximity of the inner wall of the channel, it was thought that investigation of the frequency of the eddying might give interesting data on the extent of the channel interference on such a flow.

The nozzles from which the fluid flowed are shown in section in Fig. 8. The earlier experiments were conducted by the writer alone at East London College, but the work was continued on removal to University College, in collaboration with a colleague, Mr. E. Tyler. The nozzles were submerged mouth downwards in a large rectangular tank filled with clear water. Water coloured with Meldola's blue issued through the nozzle from a reservoir into the clear water. The type of flow is shown in Fig. 9. V was measured by collecting the water from an overflow pipe, which met the surface of the liquid at right angles, near the top of the tank; n was found by counting (when less than 6 per second), or by stroboscopic methods.

The problem of the dependence of the eddy frequency on the dimensions of the nozzle was attacked from two directions. At first discs of different diameter were used in three channels; this meant that both D (diameter of disc) and d (width of slit) varied. Subsequently a fixed disc was used, outer tubes of different diameters being clamped round it, so that d varied while D was constant. The results show that there is a critical velocity for each value of the width of the slit below which the fluid emerges in a steady stream. From velocities a little above the critical, the relation between V and n becomes linear, the slope $\delta V/\delta n$ of the line decreasing as d increases, tending to a limiting constant slope when d reaches 2 mm. The critical velocity is inversely as d and practically independent of the disc. This indicates a Reynolds' number of about 40 as the limit for steady motion in the slit. The type of embyro vortex ring which was observed to issue from the slit can be observed in the photograph (Fig. 9).

The fact that, when the width of the slit is considerable, the slope of the linear part of the $V:n$ curve tends to a constant limit, confirms our earlier suggestion that when the circumference of the disc is at a moderate distance from the outer tube we are observing the æolian eddies, due to the disc placed in the path of the issuing stream. The increase in the slope of these curves as the slit gets smaller must then be attributed to the proximity of the outer walls.

To confirm these conclusions, values of V and n for a disc in an open water channel were obtained.

If we regard the phenomenon as an æolian eddy effect, modified by interference of the outer tube of the nozzle, we must adopt a formula of the type:—

$$\{ V/n\,(D-D_0) \} \cdot f\,(d/D) = \text{constant}.$$

Fig. 8.

FIG. 9.

The form of the function $f\,(d/D)$, which expresses the influence of the boundary, may be determined by plotting $(\delta V/\delta n)\,(1/D)$ against d/D. This is done in Fig. 10.

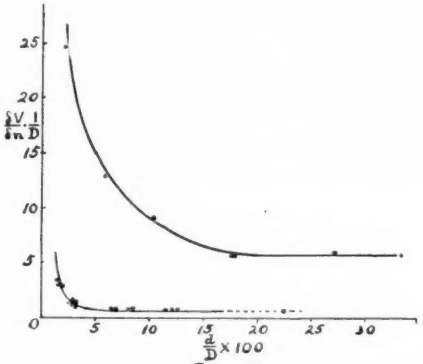

Fig. 10.

The results then fall on to two curves, one for the discs used in the narrow nozzle and one for those in the wider nozzles. It was impracticable to use the set of small discs in the wide tubes owing to the impossibility of getting

symmetrical flow when d exceeds 4 mm. That all the results do not fall on an unique curve, though each curve has the same shape, is due to lack of constancy in $\delta\Gamma/\delta n \,.\, D$, even in an open channel. If we insert a $D_0 = .25$ c.m., we get agreement in the horizontal part of the curves, just as was suggested for the wires. Having only the two curves, this value of D_0 for the discs must be regarded as very rough. At any rate, these curves should indicate to what extent channel interference should be expected in experiments on vortices and allied phenomena.

Rotating Cylinders

In view of the attention attracted to the Magnus effect by the trials of the Flettner rotor ship,[17] it was thought of interest to extend the investigation on the Bénard vortices behind stationary cylinders to those produced when the cylinder is rotated about its axis. There is, of course, no net circulation round a stationary cylinder, but when the cylinder rotates a circulation of value $2\pi rW$ is set up which produces the well-known cross force which provides the motive power of the rotor ship.

Tests of the value of this cross force have been made in a number of aerodynamical laboratories,[18] originally in Göttingen. It is found that the force actually obtained is considerably less than that predicted by theory, but that better agreement is obtained by providing the rotor with end discs, which obviate some of the loss of circulation in the form of trailing eddies at the ends. Fig. 11* shows a model rotor ship and schooner tested in the Göttingen wind channel. In spite of the meagre surface of the rotors of the former, these two models were found to have the same effective "sail area" when the rotor was rotated at a peripheral speed four times that of the channel.

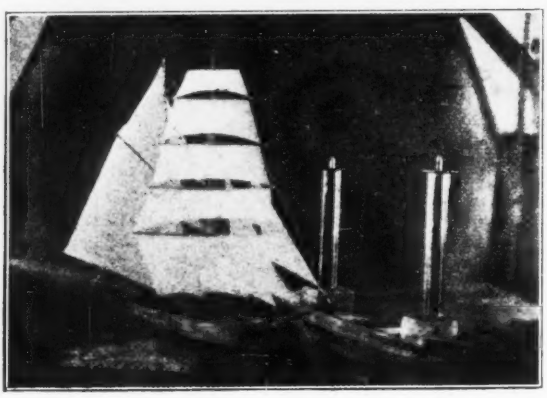

FIG. 11.

I have a model here to demonstrate the Magnus effect. Falling weights rotate a horizontal cardboard cylinder through the medium of threads. When sufficient speed has been attained I can release the "rotor" from its bearings. Note that the cross force causes it to pursue a curved path instead of falling direct to the ground.[19]

It would have been very difficult to observe the actual *vibrations* or æolian tones of a rotating rod or wire; instead it was thought better to measure the frequency of the eddy production by the cooling effect on a hot wire. This

* *From* Flettner, Z.F.M., 16, 52, 1925.

method was devised by Relf and Simmons[20] to obtain values of V/nD behind a fixed cylinder up to high values of VD/ν. In the present research an electrically heated platinum wire (1in. long and .001in. diameter) was mounted horizontally behind the obstacle in a wind channel and was connected in series with the primary of a step-up transformer, the secondary of which formed part of a shunted vibration galvanometer circuit. If the frequency of heating and cooling of the wire, which equals the frequency of the production of vortices, also equalled the tuned frequency of the galvanometer, a large oscillation of the string of the latter was exhibited. Moreover, and this was the advance made on the method of Relf and Simmons, if the hot wire was placed in different positions behind the cylinder, the extent of the heating and cooling should be greater when it is placed in the path of the cores of the vortices than on either side, and accordingly the response of the galvanometer would be a maximum if the hot wire were placed in the line of centres of the vortices. Then the hot wire and vibration galvanometer should serve as indicators of (1) the paths of the disengaged vortices, (2) the relative intensities of the vortices on either side. The latter quantity is of importance if, as Prandtl[21] suggests, the circulation of the rotating cylinder is established at the expense of the vortices on one side.

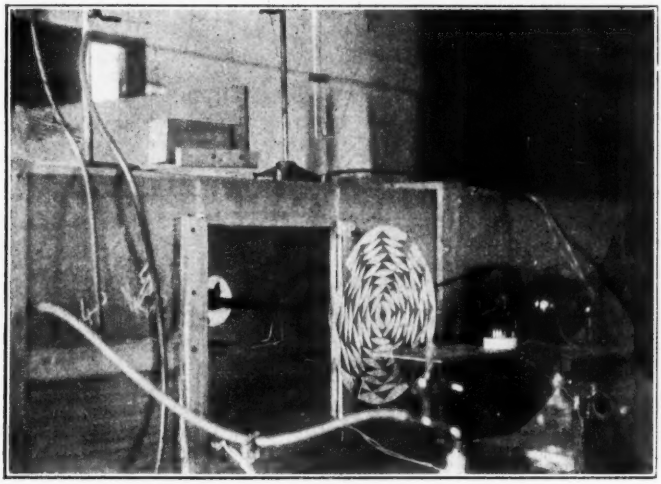

FIG. 12.

The delicate practical work involved in these experiments was kindly undertaken by Mr. Tyler. The first requirement was a wind channel; as there was at that time (1924) no such apparatus at University College, and no large funds to meet the expense of a large wind channel, by the kindness of Professor Porter we were enabled to construct a 12ft. × 1ft. 6in. × 1ft. 6in. channel of wood in the Carey Foster Laboratory, using an existing ½ h.p. motor and an exit cone of cardboard. The maximum wind speed is 500 cm. per second and is measured on a Chattock gauge, via the usual "static hole." The cylinder (a wooden rod) was supported horizontally in the channel and provided with end discs. It was driven from a motor outside and its speed of revolution was measured stroboscopically. The complete apparatus is shown in Fig. 12. The channel being run at constant speed and the cylinder stationary, the electrically heated wire was placed horizontally behind the cylinder and on a level with its upper surface. The galvanometer was tuned to best resonance with the periodic

cooling, so as to get the maximum response. The frequency of the response with the tuning slide in any particular position was known from a previous calibration. The wire was then traversed across the channel in a number of vertical planes behind the cylinder. Fig. 13 shows a typical curve of response with two peaks at a distance h, somewhat less than D, apart, due to the positions of the central vortices. Note how the rotation falls off on either side of the two peaks. It is important to remember that the transformer and vibration galvanometer system registers only the extent of *fluctuation* of the velocity and not the steady value of this. Thus, with the cylinder removed from the channel and the wind in the latter running at full speed, no response can be observed on traversing the channel, except close to the walls. There is a slight change in h as the vortices leave the cylinder, they approach each other a little at first and then widen out (Fig. 14).

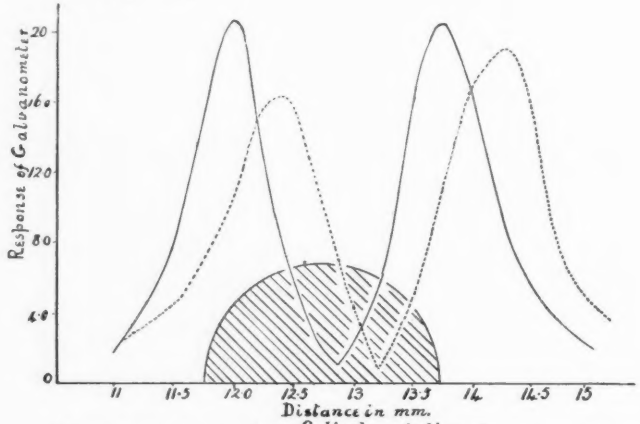

Fig. 13.

When the cylinder rotates two phenomena arise which are very important from the point of view of hydrodynamics. Firstly, the velocity at B (Fig. 2) is increased over that at B' (clockwise rotation), therefore the pressure at B is less than that at B' and a resultant force or "lift" tends to move the cylinder in the direction of B. Secondly, the particles of the boundary layer passing over B are given sufficient kinetic energy to carry them farther towards C, while those on the other side cannot get so far towards C, in other words, the secession point A is pushed farther astern, with consequent reduction of vortex production on this side, whereas A' is brought aft. If the cylinder rotates sufficiently fast A is pushed to O and no vortices are formed on this side, but compensatingly "strong" vortices arise on the under side and pass down the stream in single file. This stage can be seen in the recent photographs of Tietjens.[22]

Records made with the hot wire detector are shown in Fig. 13. Note that the peak on the side where the motion of the boundary opposes that of the stream is greater than that on the other. The over-all intensity of vorticity and therefore the drag is also reduced. We were not able to get sufficient peripheral speed to "wipe out" completely one of these vortices. The paths of the vortices as they leave the cylinder were traced out by the hot wire detector and are shown in Fig. 14.

The experiments show how rotation can be employed to modify the vortex production in the rear and provide a considerable lift/drag ratio. The cylinder

was provided with end discs in order to obviate the dragging effects of the walls of the channel.

Aerofoils

The æolian tones of brass model aerofoil sections have been determined by the revolving tank methods previously outlined, principally with a view to obtaining values of the critical Reynolds' number for periodic detachment of vortices to occur. In order that the sections should oscillate freely under the action of the vortices, they were soldered in short lengths to a springy steel strip, so that only the aerofoil dipped into the water, while the other end of the strip was fixed in a clamp. To examine the vibrations of an aerofoil in air, rubber bands were ground down to the required section by an emery wheel, giving a cord 12 cm. long and 0.2 cm. maximum thickness when unstretched.

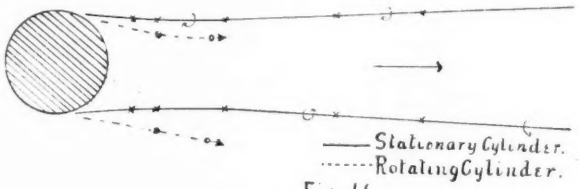

Fig. 14.

When the cord was stretched in the whirler to several times its own length it gave a note, when plucked laterally, of frequency between 50 and 130, as determined by the stroboscope. But the tone was too muffled to be heard distinctly when the driving motor was running. It appeared that to devise any visual method for testing whether the whirled cord was vibrating would be fraught with difficulty; but the problem solved itself as soon as the arms were rotated. When the cord was carried round the axle like a stiff rod the appearance was that of a dull red band between the highest and lowest position of the cord, this being due to persistence of vision. When the cord was vibrating as well as moving round an axis, it became momentarily stationary, relative to the arms at each extremity of its vibration, but moved more or less rapidly at every other phase. The extremities of each excursion of the cord were thus more marked to the eye than any intermediate position of the cord and appeared as a series of horizontal ribs against a dark background. The incidence and cessation

TABLE 5.

D	n	V	V/nD	VD/ν
.065	206	375	28.0	162
.075	120	243	28.0	121
.085	96	130	15.9	74
—	108	128	13.8	72
—	216	235	12.8	133
.095	120	180	15.8	113
.115	96	283	25.8	220
.13	86	318	28.5	276
·330	27·5	95	10.4	211
—	31·7	102	9.7	228
·337	30.0	87	8.6	198
·775	22.5	108	6.2	567
·791	22.5	138	7.8	740

of the vibration were easily detected by watching for the appearance and disappearance, respectively, of these "ribs," and further, as every two ribs represented one vibration, a check on the previously determined frequency could be made. In every case it was the frequency proper to the cord under the prevailing tension.

A correction has been applied to V to allow for the drift produced by the axle and cordholders at the small distance from the axis at which the cord was placed (about 15 cm.). It was necessary to place the cord nearer to the axis than the original wires, because the low frequencies of the former required correspondingly low velocities; too low, in fact, for the wind tunnel to be used, where drift would not have arisen. Results are shown in Table 5; the lower set were obtained with a fixed aerofoil in the channel by the hot wire method. The change of V/nD with D is even more marked than with the cylinders.

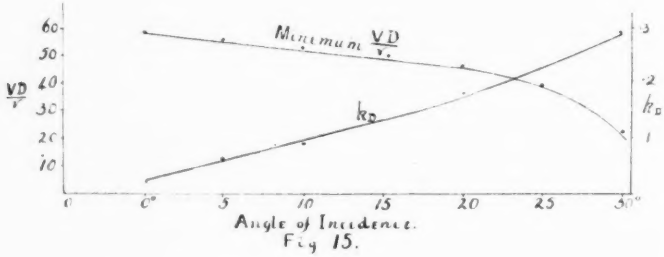

Fig 15.

As the resistance of an aerofoil varies with the angle which the flat undersurface makes with the direction of flow at a point in the fluid considerably in advance of the aerofoil, it was thought that it would be interesting to find how the critical VD/ν varied with this angle of incidence.

In order to vary this angle in the tank experiments, a little attachment was made consisting of a solid and a hollow cone fitting tightly together, each having a short pointer and a slot, in which were clamped the lower end of the blade and the upper end of the aerofoil respectively; this apparatus was only 2 by 1 cm., exclusive of the pointers. The pointers were first set radially to the tank and the blade clamped firmly to its stand above. The cone on the aerofoil was then turned five degrees at a time and observation made. In this way the vibration was always constrained to be at right angles to the stream, while the angle at which the latter was presented to the "flat" of the aerofoil was given by the angle between the pointers.

Yawing on the whirler was simply effected by drilling three other sets of holes in the arms to carry the tension clamps, inclined to the original set at 10, 20 and 30 degrees respectively. The minimum values of VD/ν are shown in Fig. 15 together with the drag coefficient of the aerofoil.

At certain higher speeds torsional vibrations of the rubber aerofoil were observed when the latter was stretched in the wind channel at a small angle of incidence. These correspond to the torsional oscillations of streamlined struts of aeroplanes observed by Harris.[23]

It was considered very important from the point of view of the theory of the aerofoil to measure the relative intensities of the vortices on either side of the aerofoil. The idea that the circulation of the *rotating cylinder* which is the direct cause of the "lift" diminishes the net circulation in these vortices has already been mentioned and is more or less confirmed by the data on Fig. 13. Prandtl[1] and Bjerknes[24] suggest that the same theory applies to the aerofoil. If then

the lift on an aerofoil is due to a circulation of this kind we should expect *prima facie* an asymmetry in the intensity of the vortices. No such is observed however. Mr. Tyler's traverse with a hot wire behind an aerofoil gave a response curve corresponding to the stationary, not to the rotating cylinder. The vortices were of equal strength on either side, though relatively closer (b less) than for the cylinder.

Airflow Through an Airscrew

Measurements of the airflow through the disc of an airscrew have generally been confined to measurements of the *average* wind velocity and direction over the whole period of the rotation with one notable exception. Drzewiecki,[27] by means of a stroboscopic pitot tube, determined the magnitude of the wind velocity in an axial direction from phase to phase of the period. As these experiments have not been confirmed it was thought of interest to use the hot wire anemometer to plot the variations in magnitude and direction of the air speed over a complete revolution of the airscrew.

At first the hot wire was simply connected to an Einthoven string galvanometer and a photographic record of the response of the latter taken. It was hoped by this means to measure the change in resistance of the heated wire placed close behind the airscrew and from this to calculate the wave form of the air speed round the "disc." At first, a test of the method was made by fixing a hot wire to the piston of a small reciprocating engine, so that the wire moved horizontally through the air in approximate simple harmonic motion; the velocity of the draught upon it could then be calculated. The wire was shielded from the "drift" of air produced by the engine itself. It was soon found that the response of the galvanometer was not a genuine record of the variations in wind speed, in fact, the resistance changes in the motion corresponded to a smaller velocity change than that existing, because the heat changes in the hot wire were too slow to follow the rapid changes in the draught. In place of this "continuous record" an instantaneous method was then developed.

The principle of the new method is that the heating current is turned on to the hot wire for a fraction only, but always at the same phase of each cycle of the airscrew, and at the same time the resistance attained is measured on a Wheatstone bridge. This resistance will depend on the time for which the current is supplied to the wire, and the velocity of the draught considered as constant over this small fraction of the period of revolution. Even if the law of this dependence is not known, the instrument can be readily calibrated *under the same conditions* in a steady constant draught. For example, if the airscrew rotates at 10 revolutions per second and the current is supplied to the hot wire behind it, always when the airscrew is 30 degs. in advance, the bridge can be balanced. The hot wire can then be placed in a wind channel with current from the same battery supplied to it by the contact maker rotating at 10 revolutions per second as before, and the channel wind speed varied till at a certain speed U and the resistance attains the same value as when the wire was behind the airscrew. Then U is the speed of flow through the "disc" at that phase in the revolution of the airscrew. By successive alterations of the phase of contact, the instantaneous velocity right round the "disc" may be plotted.

Fig. 16 shows typical calibration curves of this anemometer with the contact maker rotated at different speeds in the steady wind of the wind channel. The resistance decreases on the whole with increase in the frequency of interruption as the current has less time to warm the wire. As with the common hot wire anemometer, the instrument is most sensitive at low wind velocities, below 100 cm. per sec.

This type of anemometer can be applied to the measurement of any periodically varying air current by arranging that the contact maker shall revolve

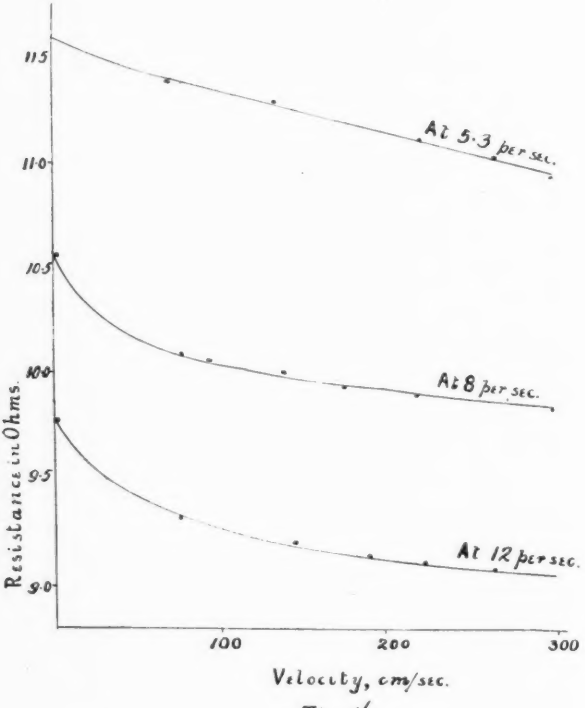

Fig. 16.

with the same periodicity. As a test, the anemometer was first applied to systems in which the periodic variation could be *à priori* calculated. In the first test the hot wire was whirled around in the draught of the wind channel by a smaller edition of the apparatus of Fig. 3. The contact maker was placed outside the

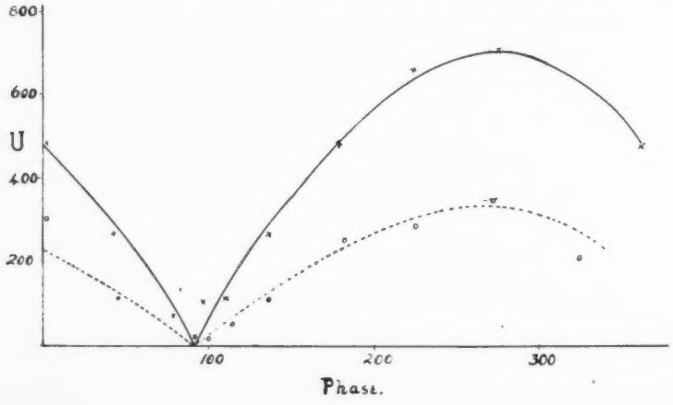

Fig. 17.

wind channel on the axle of the whirler, so that it could be set to record various phases of the motion. The calculated velocities are shown by lines on Fig. 17. The velocities given by the resistance values co-ordinated with Fig. 16 are shown by dots and agree well with the values calculated *à priori*.

The apparatus was now ready to be applied to an airscrew. This consisted of a single pair of aerofoils of the shape previously used, supported on the end of a steel axle, driven by a small motor. The axle also held the contact maker in the form of an ebonite disc, having a brass contact *B* let into it and forming one-twentieth of its circumference. This made contact between two thin brass blades projecting from the wall of the channel, thus turning on the current to the hot wire *W* and connecting it into the Wheatstone bridge system (Fig. 18). The latter was balanced by "shunting" one of the resistance arms by a resistance from the dial box *S*. Great trouble was experienced at first in keeping the speed of revolution of the airscrew constant and excessive fluctuations of the instantaneous resistance were observed, making it impossible to reach a balance. Finally a phonic motor, controlled by a tuning fork, was installed in place of the original driving motor, and as this was not able alone to drive the airscrew, the power was supplied by a larger motor from outside the channel, connected to the other by a belt. Then the outside motor supplied the power and the little phonic motor controlled the speed of the airscrew. The complete apparatus is shown diagrammatically in Fig. 18.

The results obtained with this airscrew and anemometer cover the following aspects:—

1. Air flow through the "disc" at constant incidence, but varying speed of revolution (Fig. 20).
2. Air flow through the "disc" in different planes behind and before the airscrew (Fig. 19).
3. Air flow in different "working states."

Curves are shown of the wave form of velocity round one half of the airscrew "disc" when rotating at 300, 600 and 900 revolutions per minute. The airscrew was made to rotate in a channel wind nearly equal to the average wind which it could produce working alone, so that the screw advanced quite slowly through the relative wind and the "slip" was small. These conditions were adopted because the anemometer is most sensitive when the absolute speeds to be measured are small. Measurements of the resistance attained were made step by step round the "disc" and converted to velocities by subsequent calibration in the steady wind of the channel, after removal of the airscrew from its axle, everything else being left *in situ*.

The aerofoils forming the blades were set at 15° to the plane of rotation. Negative phase angles correspond to epochs *before* the blade had passed in front of the anemometer. The leading edge of the aerofoil passed at −10°, the trailing edge at 0°.

By using a double hot wire instrument in which the two wires are placed close together and parallel to each other it is possible to determine the direction of the wind. Such directional anemometers have been constructed by Bailey[25] and by Burgers,[26] based on the principle that when the plane of the two wires lies in the direction of the wind the rear one is shielded from the draught by the front one and the effective resistance of the pair is raised. This shielding effect extends to a distance about five times the diameter of the front wire. The same principle can be applied to my step-by-step anemometer if two platinum wires a fraction of a mm. apart are mounted on the same holder, which can be turned in various directions to the draught. The current passes through the two wires in parallel and the holder is orientated until the combined resistance is a maximum. A typical plot of the direction of flow in a plane 2 cm. behind the

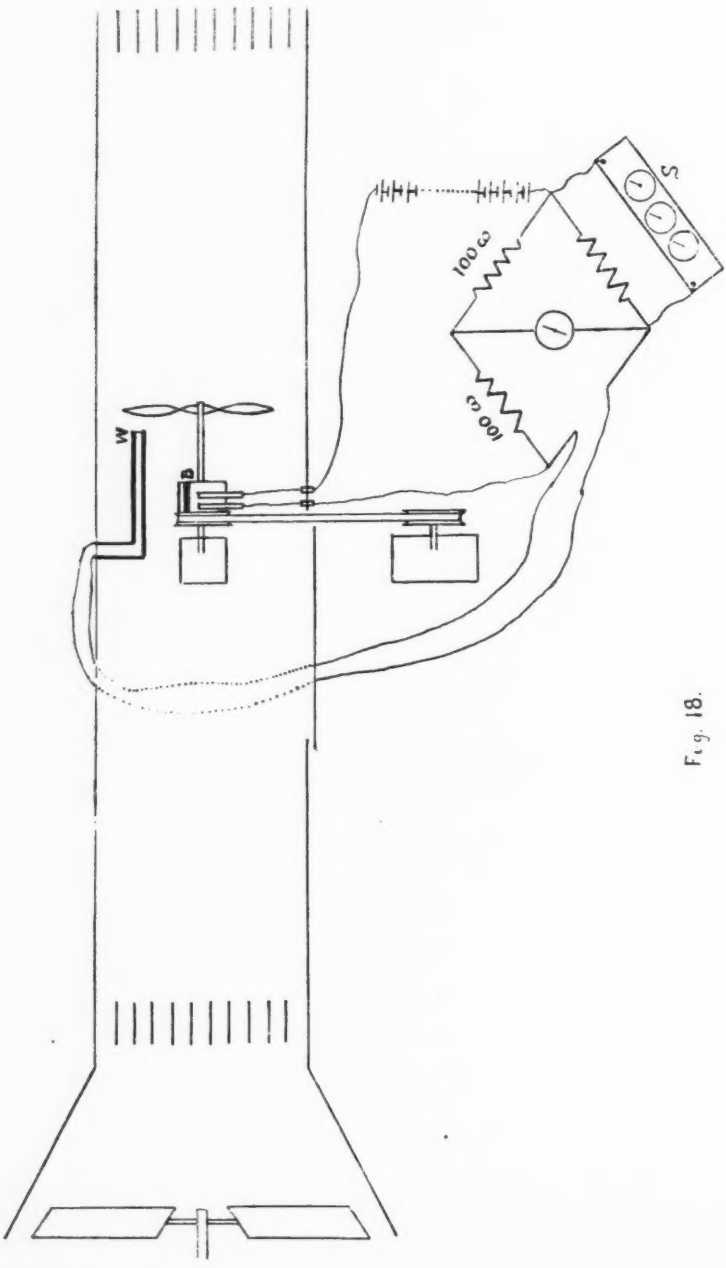

Fig. 18.

airscrew made of the single aerofoil blade is shown in Fig. 21; the length of the arrows is made proportional to the *magnitude* of the velocity at that particular phase.

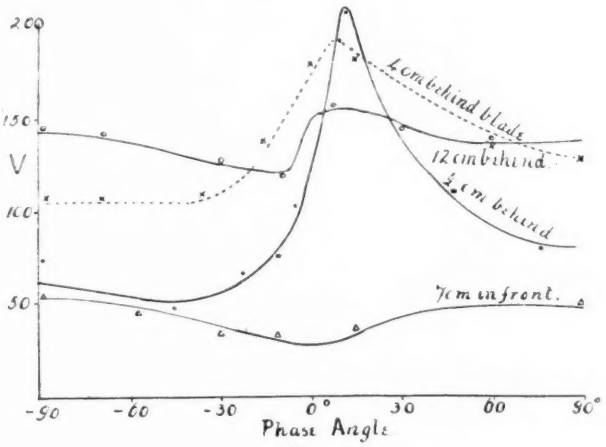

Fig. 19.

The results shown on the graphs may be summarised as follows:—

(1) As the blade passes over a given point in its revolution it imparts a large impetus to the erstwhile steady stream at this point. This impetus begins a few degrees before the blade reaches the point in question and continues a few degrees after the blade has passed, falling off gradually until the blade is about 90 degs. on.

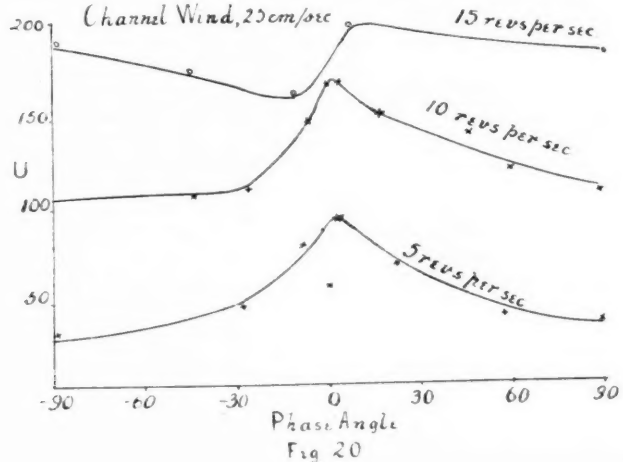

Fig 20

(2) Accompanying this impetus there is a sudden change in direction of the wind to about 45 degs. with the channel and airscrew axis, gradually reverting to 0 degs. as the blade recedes.

(3) Immediately behind the blade a momentary drop in the wind velocity is often apparent in the shadow of the blade (*cf.* Fig. 24).

(4) The impetus to the air or "inflow" is still apparent, though diminished, some distance behind the blade. Measurements in planes more than 6 cm. behind the disc were difficult as the readings fluctuated, indicating that the disturbance produced by the blades was degenerating into general turbulence. Notice the curious drop in velocity at a point in advance of the blade (Fig. 20) in place of the usual impetus.

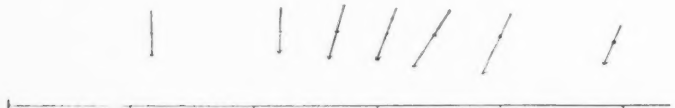

Fig. 27.

The theory of the airscrew developed by Drzewiecki[27] envisages each element of the blade as being attacked by a wind of definite velocity and angle of incidence. Knowing these two quantities and the appropriate lift and drag coefficients of the elemental section, and integrating over the whole blade, the performance of the blade as an airscrew can be calculated.

Thus let Fig. 22 represent such a section of a blade distant r from the axis of an airscrew moving through the air with a forward velocity U (or let U represent the channel wind in the case of our stationary airscrew) and rotating with angular velocity ω. At the instant pictured the element is moving at right angles to the wall of the channel with linear velocity $r\omega$.

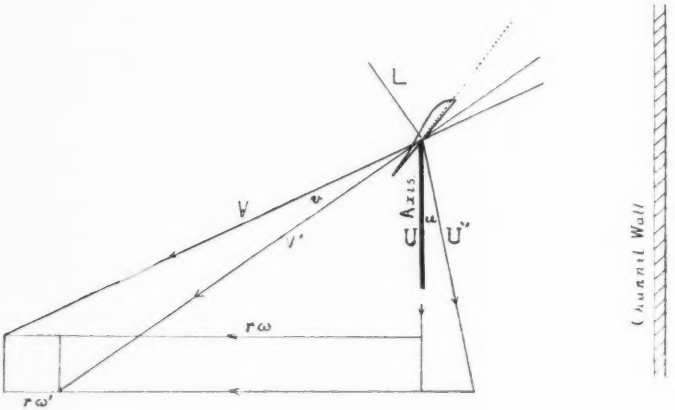

Fig. 22

In the original exposition of the theory, the wind relative to the blade element was taken to be $\sqrt{(U^2+r^2\omega^2)}$, making an angle of incidence to the blade corresponding to the resultant V of U and $r\omega$. The angle of incidence would then be that made by the direction of V with the chord, or more strictly

with the line of no lift on the blade. Actually a number of influences militate against these simple conditions:—

(1) The air flow at the instant when the air "attacks" the blade is faster than the average U (*cf.* Figs. 19 and 20).

(2) The flow is not quite axial, being inclined to meet the blade, so reducing the angle of incidence (Fig. 21).

(3) The helical vortex sheet which, according to Prandtl, is shot off from the tip of the blade tends to alter the relative wind in magnitude and direction.

Returning to Fig. 22, we must now draw V' to represent the relative wind in magnitude and direction, making an angle v with V, set off $r\omega$ perpendicular to the axis and obtain a new U' at an angle u with the original U and the axis of the channel. U and $r\omega$ are known; the measurements by the step-by-step anemometer give U' and u, hence it is possible to find the magnitude and direction of V' by the reverse construction. From known values of the lift and drag coefficients k_L and k_D, corresponding to this particular angle of incidence, the forces on the element can be calculated for

Lifting reaction of strip of blade, $\delta L = k_L \cdot \delta A \cdot \rho \cdot V'^2$ (δA being the surface area of the strip).

Dragging reaction, $\delta D = k_D \cdot \delta A \cdot \rho V'^2$.

Axial thrust, $\delta S' = \delta L \cos \beta - \delta D \sin \beta$.

Circumferential force × radius = torque, $\delta M = r(\delta L \sin \beta - \delta D \cos \beta)$; ($\beta$ is the angle which V' makes with the plane of the disc).

By integration over the whole blade the total thrust and torque on it can be determined.[28]

A number of empirical assumptions regarding the relationship between U and U', and estimations of the angle u or v have been put forward for calculating the thrust and torque. It is common to assume that the induced wind is equal to *half* the total average increment of axial velocity produced by the airscrew. The graphs justify this assumption as an approximation for about half the impetus occurs before the blade has "attacked" the air at the point where the anemometer was placed.

The graphs so far shown indicate the normal working state of the airscrew, in which the latter drives the air in the same direction as the channel fan and so accelerates the channel wind. De Bothezat distinguishes three other states:—The "zero-torque" state, wherein the airscrew, though rotating in the same sense as in the normal working state, adds nothing on an average to the channel wind; the "windmill-brake" state, in which the airscrew decelerates the external wind; and the "vortex-ring" state, wherein it opposes the channel wind to such an extent as to reverse the net direction of air flow through it.

Plots of the air flow round the disc in the two latter states are shown in Fig. 23. For the windmill-brake the airscrew was made to oppose the channel wind and nearly overcome the latter. The curve obtained is the reverse of that in the normal working state. In the vortex-ring state the measurements were very difficult, again pointing to general unsteadiness. The general effect is a very small change of velocity round the cycle.

The final figure dealing with the flow through an airscrew is an attempt to show the extent of the "interference" caused by the shedding of helical vortices from the blade tips. It was thought that this could be reduced by enclosing the airscrew—in this one case, a propeller such as is used on a model aeroplane having progressively increasing "chord" and angle of incidence from boss to tip, in place of the aerofoils of uniform chord previously used—in a circumferential

rim. The rim corresponds to the discs used on the ends of the rotating cylinder. Results are shown in Fig. 24 (upper curve).

For the purposes of obtaining this plot, the blades were made spokes to a tinplate rim, about 6 cm. long in an axial direction but very thin radially, which was carried round with the airscrew. A correction was made for the circulation

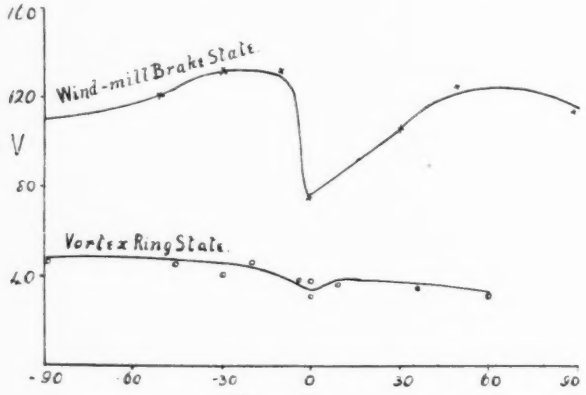

Fig. 23

of air produced by the rim alone. Without the rim, the impetus given to the air by the propeller was not so marked (lower curve).

The results generally illustrate the blade element theory of the airscrew, and indicate the possibility of determining by means of the step-by-step hot wire anemometer the extent of the "inflow" produced by a given blade, instead of making more or less arbitrary assumptions as to the magnitude of this factor.

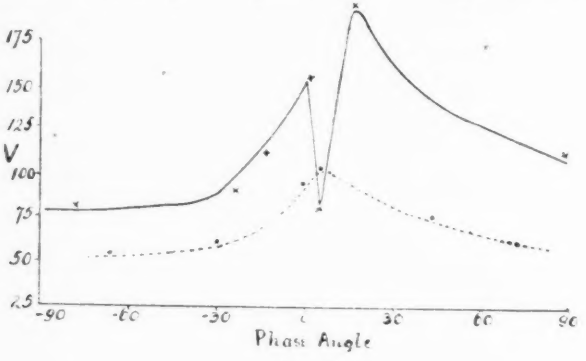

Fig. 24.

Measurement of Lift and Drag—Application to Auto-Gyro.

In order to complete these researches it was desired to measure the lift and drag on the wooden aerofoil used in most of the later experiments. As the wind channel was not provided with a balance for measuring the aerodynamic forces

upon the aerofoil, it was at first thought that it would be necessary to send the aerofoil elsewhere to have its lift and drag characteristics measured. Considerations of the principles involved in Cierva's[29] auto-gyro suggested what I believe to be a novel and rapid method for determining the lift on an aerofoil, which has also the merit of cheapness and simplicity.

A small hinge can be screwed into the end of the aerofoil, and this hinge is then clamped to a vertical steel rod set up in the channel so that the aerofoil lies across the channel and is free to move about the hinge in a vertical plane. The angle of incidence can be altered by turning the screw in the hinge. When the channel is set in action, a lift L acts on the blade at its centre of pressure and at right angles to its lower surface. If the air speed is made sufficiently great, at a speed U this lift will be just sufficient to overcome the weight of the aerofoil and hold it in a horizontal position. Unless abnormal speeds are to be used the section must be of wood or similar light material. The length makes no difference if the section is uniform, since weight and lift increase in the same ratio as the length. To determine the lift coefficients, this "lifting speed" was found for a number of different angles of incidence ranging between 5° and 30°.

TABLE 6.

Angle of incidence, θ.	5	10	15	20	30
Lifting speed, V	610	600	472	505	405
Lift coefficient, k_L	·37	·39	·62	·545	·35

At the lifting speed V

$$k_L = Mg/\rho AV^2$$

where M = mass of blade = 6.2 grm.
g = acceleration due to gravity = 981 cm./sec.
ρ = density of air = 0.0012 grm./cm.
A = area of under surface of blade = 37 sq. cm.

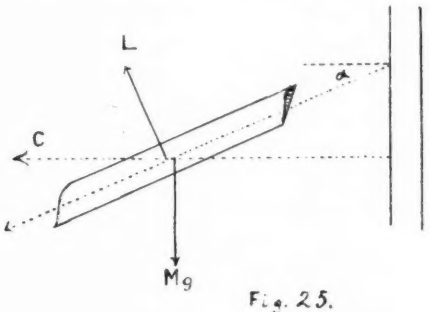

Fig. 25.

It is also possible to determine the drag if the vertical steel rod is free to turn in bearings and a couple be applied to it sufficient to keep the aerofoil edge-on to the wind. The drag coefficients are shown on Fig. 15.

If we put a pulley wheel on top of the rod and connect this by a belt to a small electric motor which whirls the hinged blade round the vertical pillar, we have a single-bladed model of the lifting airscrew of the auto-gyro. To the two forces acting on the blade there is now added a third, the centrifugal force, acting outwards from the centre of gravity of the blade. The disposition of the forces is shown in Fig. 25; it is assumed that the centre of pressure lies close to the centre of gravity of the blade.

If the aerofoil "attacks" the air at an angle θ, then the forces, resolved in the plane of the paper, are at right angles to the edge of the aerofoil, $C = M\omega^2 r$

at right angles to AX and Mg parallel to AX. Under the action of these forces the aerofoil sets at an angle α with the horizontal, such that the resultant lies along the aerofoil and passes through the hinge. Under the circumstances

$$L = Mg \cos \alpha - C \sin \alpha.$$

In the auto-gyro the axle AX of the supporting windmill is set back from the vertical and the rotation is produced automatically as the system is driven through the air. This is because the blade attacks a relative wind compounded of the speed of translation with the speed of revolution of the airscrew, so that the resultant force on the blade is inclined to the line of the axle, by an angle depending on the relative wind. Consequently, above a certain critical speed of revolution, produced by manual labour on the present form of auto-gyro, the resultant force is sufficiently inclined to carry the blade forward, and the airscrew auto-gyrates. Probably the asymmetry of the "field" of air flow produced by the rotation helps to maintain it, once started. Belort[30] has recently noted the auto-rotation of a disc hung up in his wind channel so as to be able to turn freely about a vertical diameter. On giving the disc a twist it continued to rotate in either direction. Wind channel experiments by Seiferth,[31] at Göttingen, indicate that the total L D ratio on the gyro disc is greatest when the axis is set back about 30° from the perpendicular.

As the system under test consisted of only one blade, I was forced to rotate it by independent power. In consequence of the greater relative wind on one side the lift is greater than on the other and the aerofoil beats the air as it is carried round the axle. This beating was not noticed in the Göttingen experiments as the centrifugal force was too great compared with the lift, but was a marked feature of my aerofoil's evolutions. The greatest rise and lowest droop were observed in a telescope.

TABLE 7.

A. Axis of rotation vertical. $\theta = 5°$. 5 revs. per sec.

Channel velocity ...	0	195	310	430	110
Extreme values of α	$-5°$	10, −20	18, −30	28, −35	−2, −10

B. $\theta = 5°$. 3.3 revs. per sec.

Channel velocity ...	0	100	200	250	420
Extreme values of α	−12°	−30, −3	5, −40	8, −40	30, −30

C. Axis of rotation, set back 5°. Channel wind 250 cm./sec.

Revs. per sec. ...	3.05	4.35	5.5	6.25	7.7	11.0
Extreme values of α	10, −30	12, −22	15, −20	10, −13	11, −11	20, −20

(A negative value of α indicates that the blade droops.)

The maximum and minimum lift on the aerofoil occur when it lies across the channel, *i.e.*, at $\psi = 0°$ or 180°, if $\psi = 0$ represents its position when moving momentarily at right angles to and against the channel wind. The actual positions of maximum elevation are shown on the next figure, from which we observe the striking fact that this occurs at nearly 90° further round the airscrew disc, *i.e.*, when the blade is pointing upstream, though the lift is less in this position.

The discrepancy is in part to be accounted for by these considerations:—(1) The angle of incidence is periodically altered by the beating being lessened on the rising side and lowered on the falling side; (2) the drift produced by the rotation affects the relative wind making the difference between the two extremes less than the theoretical difference between $r\omega + U$ and $r\omega - U$; (3) the whole problem is a dynamical, not a statical one, making the theory very complex.

The equation of the "flapping" motion may be written:—

$$Md^2y/dt^2 = -ky - \mu dy/dt + (L_0 - L_{180}) \sin \omega t / 2\pi$$

where y denotes the vertical displacement of the blade supposed concentrated at the centre of the mass, k is the restoring force per unit displacement, μ the damping coefficient, and L_0 and L_{180} are the maximum and minimum values of the lift occurring at $\psi = 0°$ and $\psi = 180°$ respectively, "sines" of higher orders being neglected.

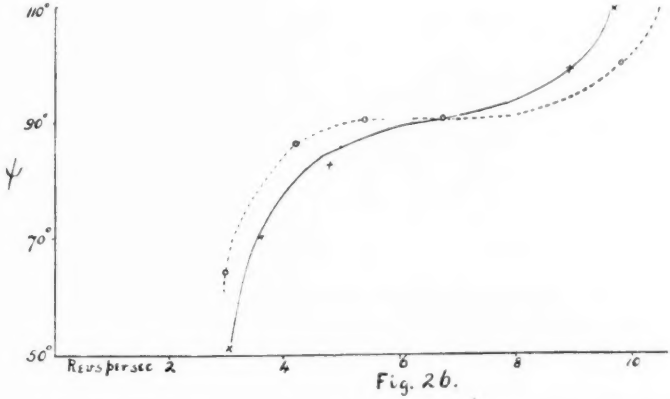

Fig. 26.

It is shown in text books on Dynamics and on Sound* that the solution of this equation is (putting $p = \omega/2\pi$):—

$$y = [(L_0 - L_{180})/\{\mu^2 p^2 + (k - Mp^2)\}^{\frac{1}{2}}] \sin(pt - \delta)$$

representing the periodic up-and-down motion under the action of the periodically changing lift; δ represents the phase lag of the flapping behind the forcing motion of the lift. It is given by:—

$$\tan \delta = \mu p/(k - mp^2)$$

so that $\delta = 90°$ when $p = \sqrt{(k/m)}$, *i.e.*, when the speed of rotation corresponds to the natural frequency of the system, in the absence of friction. There is in fact "resonance" between the forcing action of the varying lift and the flapping, the amplitude of which then becomes $(L_0 - L_{180})/\mu p$. When μ is small, δ will pass quickly from 0° to 90° as ω increases through the resonant position, but when μ is large δ rises above or falls below 90° only a little on each side of the resonant speed of revolution.

The restoring force in the case of the flapping wing is mainly provided by the centrifugal force $m\omega^2 r$ so that k/m will be large in practice. From observation it appeared also that the damping was large. These two factors together, k large and μ large, prevent the attainment of large amplitudes, represented roughly by the tilt of the plane of rotation from the normal, *i.e.*, by α in Table 7.

The figure below, which I have taken from Senor da Cierva's paper, even allowing for exaggeration by the camera, shows a blade elevated as it passes the forward position (Fig. 27).

Fig. 26, showing the phase at which the maximum rise of the blade occurs at different speeds of rotation, confirms the suggestion that the blades of the auto-gyro form a "forced system," heavily damped, under the action of the oscillating lift produced by the relative wind, and indicate the possibility of

* See, for example, p. 49 of my text-book on *Sound*.

considerable amplitudes at resonance, which may result in fracture of the blades or their hinges. I suggest this as a probable cause of the failure of the blades experienced in certain of the tests. The trouble could be eliminated by correct design if the section and the angle of incidence be so arranged that resonance between the period of rotation and the natural period of flapping at that speed of rotation does not occur. On a small scale the resonant frequencies and damping could be investigated by some such wind channel experiments as are outlined above. (Fig. 27 is a reprint of Fig. 22, p. 18 of the Journal, 1926.)

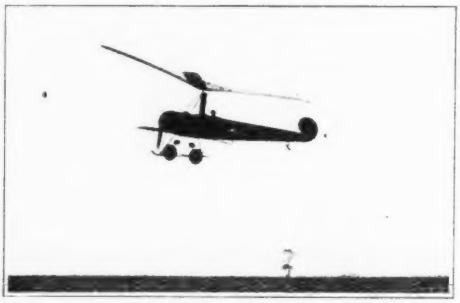

FIG. 27.

Summary and Conclusions

In this paper I have described experiments, perhaps rather loosely connected, on air flow in relation to models which I have made, but which nevertheless lead up from the fundamental ideas on the "boundary layer" of Prandtl, through the study of comparatively simple systems like cylinders and rotating cylinders and aerofoils, with sidelights on the circulation theory of the aerofoil, to complex systems like the airscrew and the supporting screw of the auto-gyro. I hope that these will be of some use to those whose business is with lifting surfaces of far greater size than the laboratory can hold and who are not so easily able to choose their experimental conditions.

I wish to give my best thanks to Professors Lees and Porter and Dr. Piercy for the interest they have shown in my work and for the facilities which they have placed at my disposal. Finally, I am grateful to the Society for this opportunity of bringing my work before them.

APPENDIX

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Discussion

Colonel the Master of Sempill: The paper we have just listened to is truly a remarkable one, describing as it does the results of five years' work of one man. The assistance that Dr. Richardson has received from other quarters has indeed been small, and our congratulations are due to him for coming here this evening and describing to us so lucidly the results of his investigations. He sets us a lesson in pertinacity, a quality which might well be cultivated more assiduously by us all

I have been very much struck by the paper, as it clearly shows the breadth of the lecturer's knowledge, and should result in others embarking on somewhat analogous research work, and it is clearly demonstrated that a great deal can be done for a very small outlay.

Dr. Richardson has been able to keep well in touch with progress in other fields, and possesses quite a remarkable knowledge of the more recent developments in Germany.

Dr. Piercy: He had lost touch with Dr. Richardson's work, except in a general way, since the time the earlier part of it was being carried out at East London College. Thus much of the present paper was new to him, in spite of the lecturer's kind references to the help he had received there.

The lecturer had attacked with singular energy and originality the somewhat difficult part of aerodynamics that had to do with the generation and motion of vortices. The importance of accumulating knowledge of this subject was becoming more evident, not only scientifically but also from the point of view of directly useful applications. Dr. Richardson had added to this knowledge, and had incidentally suggested methods of experiment that might be of use in a variety of ways. His investigation of the flow through an airscrew was also an illuminating development of Drzewiecki's work.

It was surprising that æolian tones were obtained at such low values of Reynolds' number as 30 for round cylinders, and 50 for aerofoils. The periodic motion behind circular wires had received considerable attention at the N.P.L. and eddies were thought not to occur much below $VD/\nu = 100$; indeed, drag could be fairly closely calculated by steady motion methods at that stage. Some recent measurements at East London College of the lift of aerofoils at very small Reynolds' numbers had again suggested a much larger critical value in this case. It would be of interest to know whether the minima recorded always referred to

the " lower critical velocity," and whether any other suggestions could be advanced bearing on the point.

Mr. W. O. MANNING: One thing struck me particularly, and that is the extremely low pecuniary cost at which some very valuable experiments have been carried out. It is evident that, given the necessary ingenuity, much valuable experience may be obtained from extremely simple and cheap apparatus.

There are one or two points in the paper on which Dr. Richardson might assist me. In the experiments with the propeller he used a hot wire instrument behind the blade. There ought to be a distinct time lag before the wire takes up a steady temperature, and as the air velocity is varying rapidly there may be considerable damping.

In the last photograph of the autogyro which he showed to support his contention, a great deal may depend on the precise camera with which it was taken. If a focal plane shutter was used, the distortion caused by this type of shutter may be quite sufficient to vitiate the value of the photograph. Although the actual exposure of any part of the plate may be only 1/500 sec., say, the time interval between the exposure of each end of the plate may be many times longer, distortion of most moving objects is inevitable. I have myself taken a photograph of a rapidly moving propeller in which the propeller was curved like a boomerang owing to this cause.

Mr. FAGE: I have been engaged at the N.P.L. on model experiments in aerodynamics for several years, so that I am naturally interested in Dr. Richardson's paper and the problems he considers. He has covered much ground. I must confess that as I read the paper I had the same feeling I imagine a bee has as it darts from flower to flower, sipping a little honey here and there, but never pausing to have a good look at the flowers. We must, however, admire Dr. Richardson's industry and his ingenious experimental devices. There are a few remarks I should like to make. According to my advance proof of the paper, Dr. Richardson says in the first section on the flow around a stationary cylinder that " the fluid which actually passes in contact with the body has, at any point, the same velocity as that at any point in the body of the fluid on a line at right angles to the surface, and therefore slips past the surface without any retardation." This statement is incorrect. I have in the course of my work determined the velocity distribution around an elliptic cylinder immersed in an inviscid fluid, and this shows that the velocity is not uniform along a normal to the surface, especially when the surface has a large curvature. Further, the streamlines given in Fig. 1 do not support Dr. Richardson's statement. I am very interested in the experiments made to measure the frequency with which vortices are shed behind a cylinder. We have just completed experiments to measure the frequency with which the vortices leaving the sharp edges of a flat plate pass downstream. For this purpose, a hot wire was used in conjunction with an Einthoven galvanometer and photographs taken of the velocity disturbances. These records show that the amplitude of the velocity disturbances is more regular outside the vortex street and also that the frequency of the disturbances within the street is double that outside. This is not in agreement with Dr. Richardson's suggestion that the best place for the hot wire is in the path of the vortices. Fig. 14 showing the path of the vortices as they leave the cylinder is interesting. Our method of determining the lateral extent of the vorticity was not so simple as that used by the lecturer; in fact, we were forced to the conclusion that the problem was somewhat difficult. Dr. Richardson refers to the calibration of a hot wire attached to the piston of a small reciprocating engine and that he found the response of the galvanometer was not a genuine record of the variations in the wind speed. This is not surprising if the frequency were high. May I inquire at what frequency the calibration was made? Dr. Richardson has shown on the screen photographs of the vortices behind a

swinging pendulum, which, I must admit, conveyed very little information to me. I wonder if the lecturer noticed any effects due to surface waves. We have tried this method and found it unsatisfactory because of the disturbances due to these waves. I gather from the remarks made at the end of the section on the interference of the walls of a channel on eddying flow that the lecturer regards the phenomenon of the flow through a circular orifice as an æolian tone effect. If this is so, what is the note, since the disc can vibrate in an infinite number of modes. In conclusion, I should like to thank Dr. Richardson for his interesting paper.

Mr. E. TYLER (*communicated*): Those concerned with aerodynamic research will welcome Dr. Richardson's paper as a most valuable contribution to this branch of science and is an example how one with simple apparatus is able to investigate "fundamentals" which in the main govern the behaviour of complicated problems, both in fluid motion and allied phenomena.

I regret my inability to attend the meeting of your Society this evening, but have read with great pleasure the paper presented by Dr. Richardson. It was at his suggestion that I undertook to investigate some of the effects recorded. With reference to the results in Table V. the lower set were obtained with a fixed aerofoil in the wind channel set at 0° to the impinging windstream. I have extended this research by investigating the effect of varying the angle of inclination of the aerofoil to the windstream and obtained corresponding values of V/ND and VD/ν. A typical set of results are given below for a modern aerofoil $D = .337$ cms. Length = 38 cms., width = 2.50 cms.

Angle of Inclination of Aerofoil θ	N (Frequency of Vortices)	V cm sec	V/ND	VD/ν
0°	30	87.0	8.60	1.98×10^2
5	30	105.0	10.38	2.39 ,,
10	22.5	87.0	11.50	1.99 ,,
15	30	140.0	13.88	3.19 ,,
20	30	185.0	18.20	4.22 ,,
25	22.5	150.0	19.96	3.42 ,,

It will be noticed that as the inclination (θ) of the aerofoil is increased, V/ND also increases, but the relation between V/ND and VD/ν exhibit no strict dynamical similarity.

I have used both brass and wooden aerofoils and find both types exhibit the same general phenomena, although the values of V/ND is modified slightly, no doubt due to the roughness of the surface.

Referring to Figs. 13 and 14, time did not permit me to make a complete investigation of the paths of the vortices at greater distances behind the rotating cylinder owing to my departure from University College, London, but the results clearly show the Magnus effect, so clearly explained by Dr. Richardson.

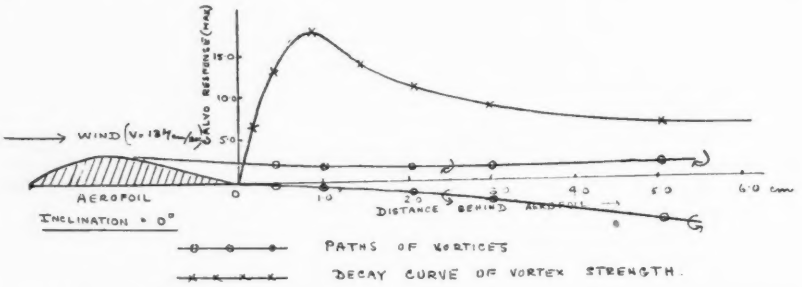
There was no change in the frequency of vortex formation for cylinders stationary and rotating.

A few preliminary results* on the paths of vortices behind aerofoils using the hot wire method I have published previously, and I hope to publish further ones shortly.

Such results show a gradual widening out of the paths; at the same time there is first the growth of the vortex and then a decay in the strength. This decrease in strength can be clearly seen in the figure below, which is a typical result obtained by traversing the hot wire transversely across the rear of the aerofoil and measuring the maximum response of the galvanometer (previously tuned to resonance frequency with the vortices) which was assumed proportional

* Journal of Scientific Inst., Vol. III., September, 1926.

to the strength of the vortex. Repeating for different distances the maximum response is plotted against the distance of wire behind the aerofoil.



The curve shows the growth and decay of a vortex as it leaves the aerofoil, and the merit of such a result is entirely dependent on keeping the heating current constant. Notice that it takes some time before the vortex reaches its maximum strength and then it seems to decay exponently. It must be remembered that working with such small scale models there is a limit to such investigations, for the vortex strengths are small and owing to the widening-out effect soon decay after travelling a few cms. along their paths, and the hot wire detector becomes insensitive to these small fluctuations of velocity which is masked by general turbulence.

My results for both wooden and brass aerofoils show generally vortices of equal strength being detached. I have not been able to detect any change in the strengths as in the case of the rotating cylinder, and the results suggest the vortices are in the main due to ordinary "æolian tone" phenomena, the effective thickness D of an aerofoil being modified as its angle of inclination to windstream is varied, and since the stagnation points are symmetrically placed with respect to the trailing edge, yet at different distances along the boundary from the leading edge, these conditions tend to maintain equal vorticity on both sides.

In conclusion, I would like to congratulate the author in presenting such a valuable paper to the Society and hope that such experimental data will provide a basis for a wider extended work in this line of research.

Reply to Discussion

Dr. Piercy: Bénard concludes that the critical Reynolds' number is itself a function of the viscosity. The minimum value of 30 occurs with the molasses mixture which has a viscosity greater than that of air. I have, however, obtained vibrations at $VD/\nu = 50$, or even 40, not only in the viscous mixture but also in air. Nisi and Porter give 10 as the critical Reynolds' number for the formation of the stationary vortices. I can only suggest that possibly the tendency for the wire to vibrate encourages the periodic shedding of these vortices. The standard curves of drag coefficient against Reynolds' number show that this coefficient becomes constant at about $VD/\nu = 100$, but there seems to be a kink in the curve at a lower value of VD/ν near 50 (*cf.* Relf, R. & M. 102) which perhaps represents a third critical velocity lying between the other two, *i.e.*, $VD/\nu = 100$ and $VD/\nu = 10$, and corresponding to that given by my method.

Mr. Manning: There is certainly a lag in taking up a steady temperature on the part of the hot wire in the step-by-step anemometer. The graph of Fig. 16 shows this; but the point is that whatever lag is exhibited by the wire when

exposed for a fraction of a second, over which the velocity to be measured may be considered constant, the same lag will be shown when the wire is exposed for the same time to a *steady* air current of the same velocity as that experienced during the given instant when the wire was exposed to the varying current; hence the possibility of calibrating the instrument in a steady wind, provided the exposure times are the same, *i.e.*, provided the contact-maker is rotated at the same speed. As at present constructed, difficulty is experienced with the sparking which occurs on breaking contact; moreover, owing to the small size of my wind channel the ebonite hub forms an inconveniently large proportion of my airscrew "disc," so that Figs. 19 to 24 must be regarded as indicative of the general form of the flow rather than as accurate values. I think the step-by-step anemometer, given improved apparatus, is capable of further development and use, and anyone who has better equipment than I have at present is welcome to extend the method if he thinks it has possibilities.

I appreciate the point in regard to the focal plane shutter, but take it that this does not invalidate the conclusion from the photograph that the blade does in fact pass through an elevated and forward position at some part of its revolution.

Mr. Fage: I admit the falsity of the statement with regard to the tangential velocity when applied to the cylinder. When I wrote the words quoted, I was thinking of a plane surface as the "body." I am afraid this and some other slipshod statements in the advance proof arose from the pressure of other work, and the consequent haste with which it had to be drafted. I have reconstructed the offending paragraph.

I can quite understand the frequency of a hot-wire response within the vortex avenue being double of that outside if the "potential lines" of both sets of vortices overlap the central line of the avenue. Reference, *e.g.*, to Fig. 42 of Cowley and Levy's *Aeronautics* shows that a hot wire placed in the wake would experience twice as many oscillations of air current as one outside the street, but near one row of vortices. This would not, however, prevent a vibration galvanometer tuned to the lower frequency from responding even in the wake. Always the two definite lines of "peaks" were obtained, which I still maintain, because of the symmetry of the "avenue," cannot be ascribed to anything else than to the two lines of vortex-centres themselves. A hot-wire and telephone method by which the passage of the vortices was made audible, recently devised and tested, has confirmed this. Our short experience with a galvanometer of the Einthoven type giving photographic traces convinced us that this instrument tells one too much by recording every passing disturbance, and so making it difficult to "see the wood for the trees" in the ensuing record.

The reciprocating engine was used at frequencies down to 10 per sec. Even at this frequency the hot wire did not record the whole of the velocity amplitude; the discrepancy, of course, gets worse as the frequency goes up, until the hot wire shows only a little ripple imposed on a large steady cooling (on this point, *cf.* Richards, *Phil. Mag.*, 45, 926, 1923). The wire cools more rapidly than it warms up again, and so distorts the wave-form of a periodic velocity change which has swept over it. As long as this wave-form and its frequency remain constant, we have found that the hot wire and vibration galvanometer do give indications proportional to the *amplitude* of the disturbance.

I claim that the eddying *flow* from annular nozzles is similar to the *flow* in æolian tones of wires, but I do not believe that the disc partakes in the vibration in the way that the wire does, since it is too rigid. No sound was heard during the experiments with the nozzles which might be ascribed to the disc itself.

PROCEEDINGS

Eleventh Meeting, Second Half, 62nd Session

The Eleventh Meeting of the Sixty-Second Session of the Royal Aeronautical Society was held in the Theatre of the Royal Society of Arts, 18, John Street, Adelphi, W.C.2, on Thursday, April 28th, 1927, when Major R. E. Penny, Associate Fellow, read his paper on "Seaplane Development." Colonel the Master of Sempill, Chairman of the Society, was in the chair.

SEAPLANE DEVELOPMENT

BY MAJOR R. E. PENNY, A.F.R.AE.S.

I am indebted to the Air Ministry for permission to use the material contained in the various official reports dealing with this subject, and I am directed to say that the views expressed in this paper are my own personal views, and do not in any way represent the official views of the Air Ministry, nor do they indicate the policy to be pursued.

I am indebted also to the makers of the aircraft for supplying information, and for permission to make use of this information.

I am also indebted to Dr. Coales, of the Air Ministry, and Mr. Launchbury, of Farnborough, for valuable help in the preparation of this paper.

It is impossible within the scope of one paper to deal adequately with the development of the seaplane in all its aspects, and therefore I shall make no apology for having only been able to touch on some of the more important features. The many controversial problems in seaplane design have retarded to a great extent the more rapid development of this branch in aeronautics. The aeroplane, on the other hand, settled down in the early stages to a universal type—the Tractor—owing to its preponderating advantages in general development and performance.

Historical

A rapid survey of the early attempts at flying from the water will be given. Among the earliest aircraft designed to fly from and alight on the water were a French craft by Gabardini and R. M. Fabre, 1910, and the Fabre monoplane tested in 1911. Their difficulties were considerable and their success slight, but by the end of 1911 floats were substituted for wheels on aeroplanes that were already proved to fly.

Then, in October, 1911, Glen Curtiss in America flew from the water with a Curtiss aeroplane fitted with floats.

In England, the first flight from the water was made by Commander Swann, R.N., now Air Vice-Marshal Sir Oliver Swann, R.A.F., and Mr. S. V. Sippe on an Avro biplane fitted with a 35 h.p. Green engine at Barrow in November, 1911.

During 1912 Mr. Gnosspelius built a small machine on Bleriot lines, fitted with floats on the "ricochet" type, constructed of wooden frames and covered with aluminium sheet. It was flown by Mr. Kemp.

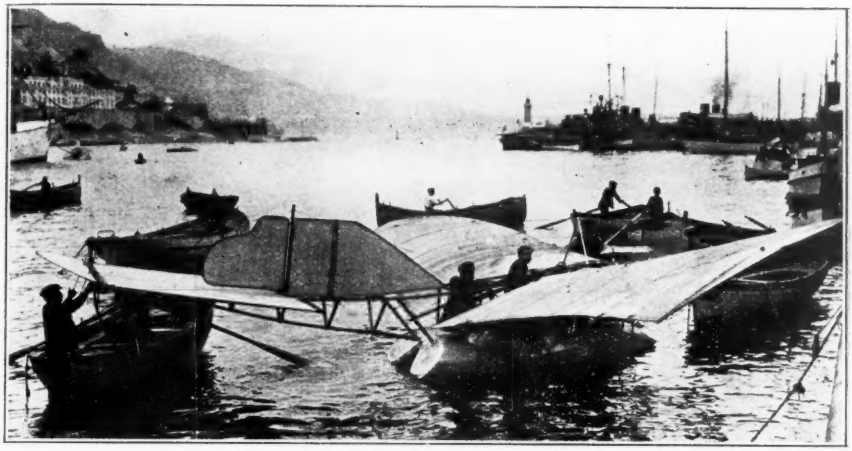


FIG. 1.
Fabre 1910, *and Gabardini* 1909-10.

In France, during 1912, Voisin (Fig. 6) and Coudron (Fig. 7) equipped their standard aeroplane with Fabre floats and flew successfully. This float was flat-bottomed, rectilinear in plan and made of wooden framework covered with proofed canvas. This type was displaced later by pontoon-shaped floats covered with three-ply wood.

The early development of the float seaplane in this country to the point of practical utilisation was due to the enterprise and energy of the late Mr. Horace Short. The first Short seaplane fitted with 100 h.p. Gnome flew in 1912 with

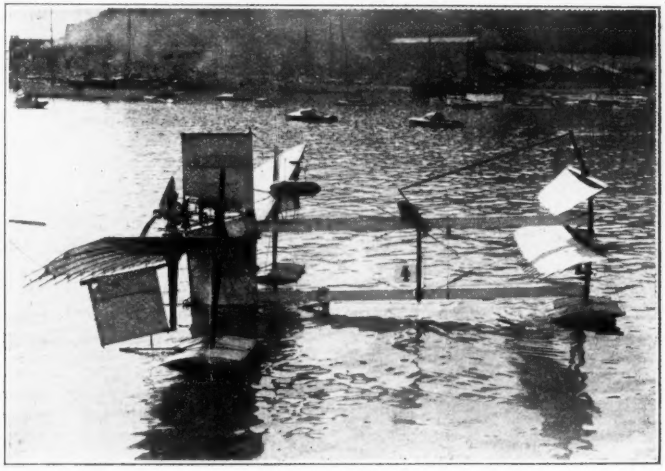


FIG. 2.
An Early Waterplane: The Fabre.

FIG. 3.
Curtiss, 1912.

FIG. 5.
Avro fitted with Floats, November, 1911.

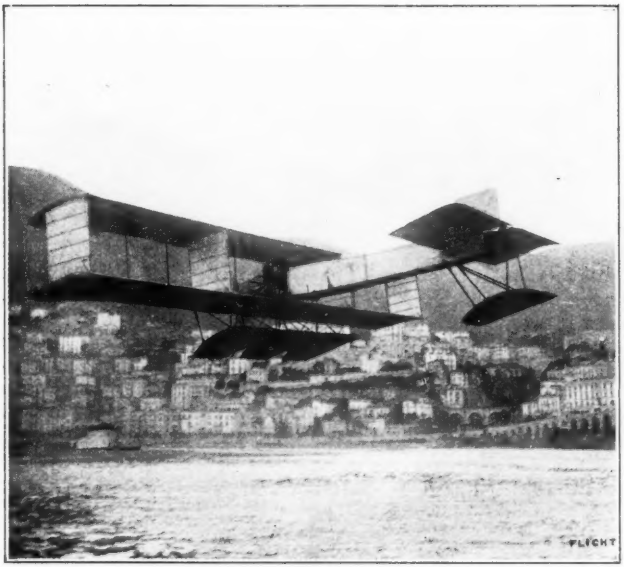


FIG. 6.
Voisin, 1912.

Commander Samson (now Air Commodore Samson). This seaplane was fitted with stepped floats, but in the succeeding design the standard flat-bottom square-sectioned float was adopted, which remained for some time the standard flotation system. About this period, difficulty was experienced with undercarriage failures

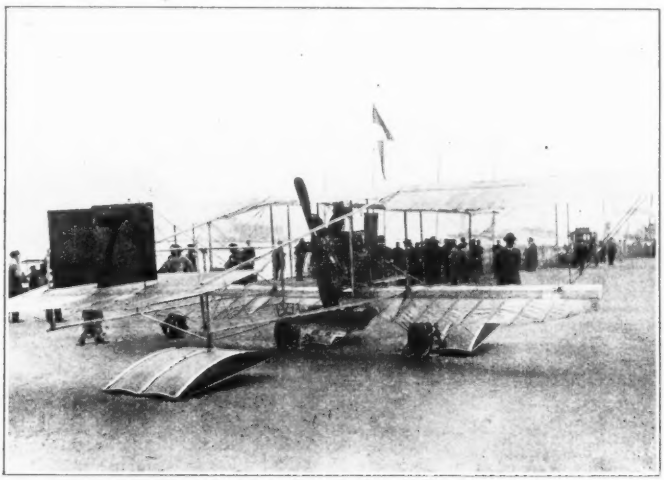


FIG. 7.
Coudron, 1912.

when flying from broken water. This led to the adoption of rubber shock absorbers between the floats and supporting struts.

The " Short " 225 h.p. seaplane, known as the 184 type, which became a standard type of machine in the Air Service until the end of the Great War, was a development of the foregoing type, being fitted with a " Sunbeam " water-cooled engine, and adapted to carry bombs and a torpedo slung beneath the float crossbars. First example flown September, 1914. One of these machines torpedoed a Turkish transport during the war, which was the first incident of its kind in warfare. A similar machine of 320 h.p. carrying a 1,000 lb. torpedo was also produced during the early period of the war.

In 1917 in the " Short " N2/B type seaplane a change in float design was made. The rectangular sectioned floats were replaced by main floats with a concave curved bottom in conjunction with a step and a streamlined rear portion. The sprung tail float was retained, but was relieved of the strain which tail floats had usually borne by reason of the greater buoyancy in the after portion of the main floats. This departure shown on this slide was a prelude to dispensing entirely with the three-point suspension float system, and to replacing it with the modern type of seaplane in which the main floats have sufficient length and buoyancy aft to keep the tail off the water.

FIG. 16.

The First Schneider Cup Contest, 1913.

The French pilot Prevost winning the contest at Monaco on the Deperdussin monoplane, 100 h.p. Gnome. His speed was 72.6 k.p.h. (45.75 m.p.h.).

The 1913 Sopwith twin-float seaplane was the first British machine to win the Schneider trophy, and with slight modification was adopted for service as a single-seater fighter and was extensively used between 1914 and 1918.

A large number of experimental twin-float seaplanes were produced during these years, but nothing that could successfully compete with the Short 184 type was evolved until late 1918, when the Fairey III.C was developed from the N.10. This machine, fitted with the Eagle VIII. engine, was extensively used during the Bolshevik campaign in North Russia during 1919.

Its later development, the III.D, fitted with a Napier Lion engine, is in service to-day and is only now being replaced by the Lion III.F.

The progress of design of float machines is strikingly indicated in the comparison of the first and last winners of the Schneider trophy race.

TABLE OF COMPARATIVE PERFORMANCES.

Date	1913.	1926.
Type	Duperdussin.	Macchi 39.
Engine	160 R. Gnome.	882 Fiat A.S.2.
Total weight	2,200 lb.	3,560 lb.
Wing area	300 sq. ft.	156 sq. ft.
Lb. per sq. ft. ...	7·34.	23.
Lb. per h.p.	13·75	4·04.
Average speed ...	45·4 m.p.h.	246.4 m.p.h.

If the engine of the Duperdussin had developed the horse-power of the Macchi, its speed would not have been more than 90 to 100 miles per hour. Roughly, it will be found that the resistance at 100 feet per second of the Macchi is less than one-twentieth of that of the Duperdussin, which shows the advance made in thirteen years.

FLYING BOATS

We will now trace the similar development of the flying boat. In 1912, Donnet and Leveque in France built and flew a seaplane, a two-seater pusher having a central hull with the engine above the boat sufficiently high under the plane for the airscrew to clear the hull. The design of this boat can be considered as a forerunner of the present-day flying boats.

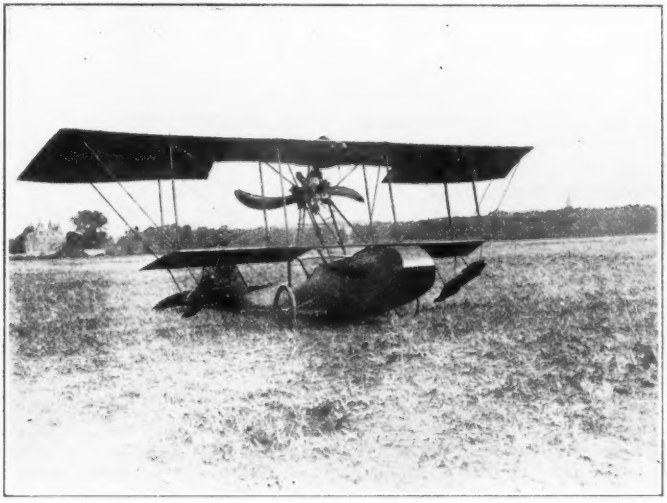


FIG. 18.
Donnet and Leveque 1912.

In America, towards the end of 1912, Curtiss replaced the single central pontoon by a boat-shaped hull which carried the tail unit. To protect the crew wood and canvas superstructure had been built on the fore part of the original pontoon, making its appearance very similar to that of the later Curtiss flying boats.

Early in 1914, the Curtiss Co. developed a twin-engined flying boat with two 90 h.p. Curtiss engines, primarily to attempt the Atlantic flight for a prize of

£10,000 offered by the *Daily Mail*. This boat, at an all up weight of 5,000 lbs., could not leave the water. A third engine was fitted which made it able to get off but reduced the air endurance, and the project of attempting the Atlantic flight was abandoned in 1914.

During the latter part of 1913 the flying boat was produced in Britain by Sopwith. It was also fitted with wheels. This aircraft won the Mortimer Singer trophy. Until the arrival of the late Commander Porte, R.N., from America late in 1914 there appears to have been little development in the flying boat in this country.

Early in 1915 several of the twin-engined Curtiss flying boats, known as the Americas, were brought over to England, but proved, however, to have poor performance mainly due to lack of h.p. and bad sea-going qualities.

From this period onwards, primarily due to the foresight and energy of Commander Porte, the development of the boat was pushed ahead at Felixstowe.

I am leaving out the remaining part of this section dealing with the war period of flying boat development, as it has already been published a number of times.

II. The Design of the Aircraft Structure in Relation to the Boat and Float Seaplane

Many of the conditions that have to be fulfilled in flying boat design are antagonistic to each other owing to the great difference between operation on the water and in the air.

Apart from this fundamental difference there are also the same general problems to be dealt with as occur in all aircraft, viz., difference between operation from harbours or aerodromes and from the open sea or temporary landing grounds, the difference between operation at sea level and high altitudes, between tropical and temperate climatic conditions. In some cases operation on high altitude lakes, for instance, in Central Africa are equivalent to conditions corresponding to 6,000ft. altitude. The landing speed and h.p. loading will be definitely limited by these conditions, which again largely influence the useful load carried. The landing speed may vary between 45 and 60 knots, and the conditions for taking off equally varied. In adverse sea conditions "landing" can even be effected where a "take-off" would be impossible. If, however, we confine our attention for the moment to the particular problem of the design of the flying boat from an aircraft point of view, certain conflicting considerations are apparent.

Regarded as an aircraft the centre of gravity of the whole and the centre of air resistance should be nearly coincident, and for this the centre of gravity should be high above the water. As a watercraft, however, a relatively low position of the centre of gravity is needed in relation to the waterplane. The compromise necessary puts the centre of gravity so that the metacentric height is negative and some form of lateral stabiliser has to be provided.

The position of the airscrew dominates the design. The air inflow near the blade tips sucks spray off the sea and picks up spray thrown up by the hull with damage to the blades. This is prevented either by putting it high up or over some part of the seaplane.

The actual arrangement decided upon will depend upon the power plant installed, and whether a single- or a multi-engine installation is used.

For simplicity and probably least structural weight, the large single engine is preferable, but a machine so fitted has bad manœuvrability on the water. The twin tandem has lower drag, less weight and less interference than the twin

outboard, and gives a good aerodynamic compromise but has a lower overall propeller efficiency and bad manœuvrability on the water. The after propeller is very inefficient if the forward one is stopped; but this could be improved if a variable pitch airscrew were fitted. The propellers, however, are well protected from solid water.

The twin outboard has a higher propeller efficiency than the twin tandem, and a high degree of manœuvrability on the water.

The three-engine arrangement, if in line, is the least efficient aerodynamically, and unless the propeller gear ratio is carefully chosen there will be a considerable loss in thrust during take-off and climb, owing to the restricted diameter required to obtain a compromise between structure weight and propeller efficiency, as the centre structure is fixed by landing loads. A flying boat fitted with three engines can take off and climb with any two engines at almost full load if the power loading does not exceed about 14 lb./h.p. and surface loading about 11 to 12 lb. per sq. ft.

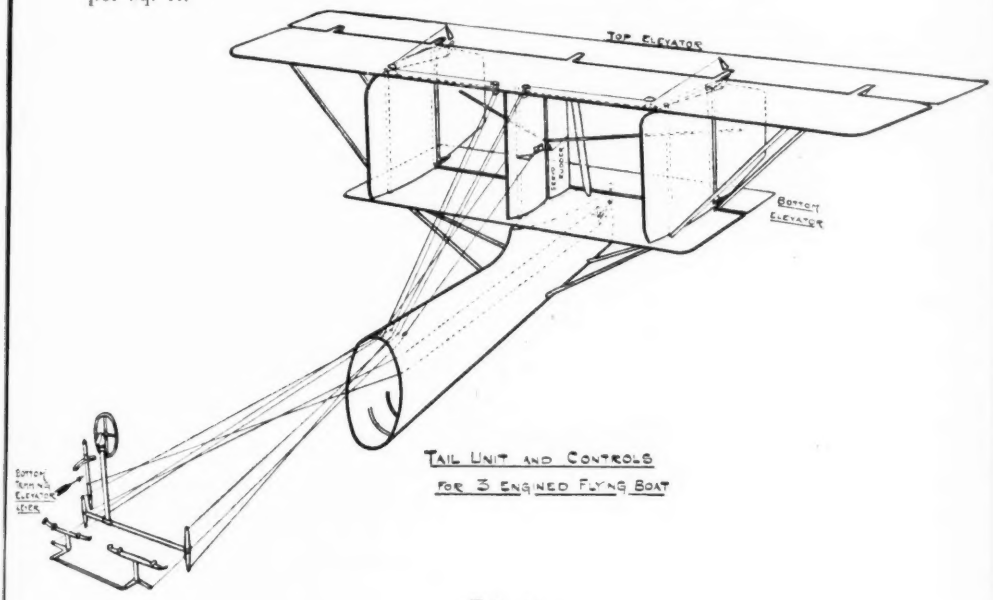


FIG. 24.

With all these arrangements, however, the fundamental requirement of effective water clearance from both waves or spray thrown up by the hull calls for a thrust axis well above the centre of gravity. This eccentricity of thrust axis produces a downward pitch varying from zero in gliding to a maximum at full power. In the earlier boat seaplane this was uncorrected, and in order to get balance in normal flight the craft was very tail heavy in gliding. By setting the tail plane in the slipstream, with a negative angle relative to the chord of the main plane, and by fixing the C.G. particularly well forward, the thrust couple is nearly balanced out by the tail couple. As the main reactions on the tail are downwards, some boats have been fitted with an inverted camber tail plane.

In order to show this more clearly, some results of wind tunnel tests carried out at the R.A.E. on the effect of slipstreams on a three-engine flying boat may be given.

Fig. 24 shows the type of tail unit from which these results were obtained. It will be seen to be of the biplane type, the upper plane of greater span than the lower. The top plane elevator is coupled with the control column in the usual way, but the bottom plane elevator is connected to a lever working in a quadrant and performs the trimming functions of an adjustable tail plane with much saving in weight and complications.

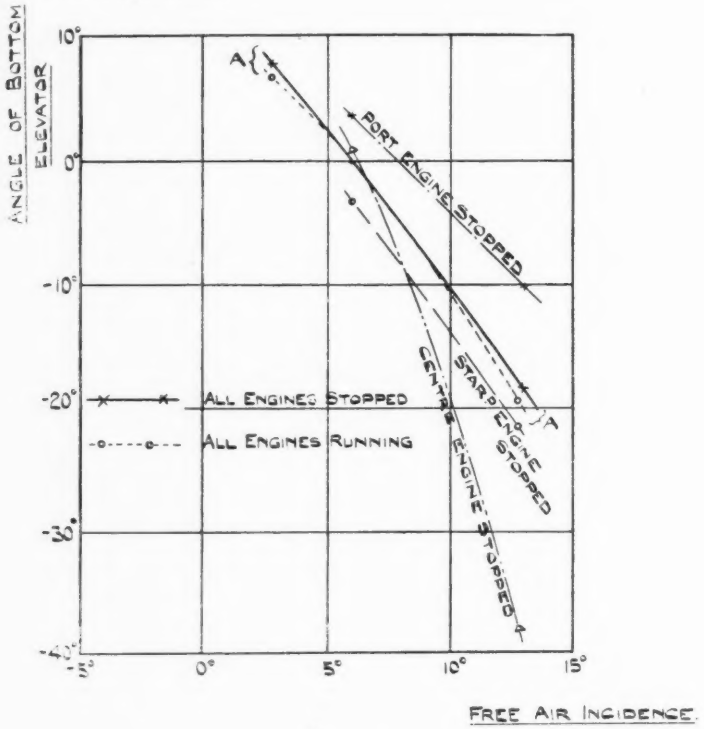


FIG. 25.

Bottom Elevator Angles to Trim with Top Elevator at 0°.

The curves marked A (Fig. 25) show the bottom or trimming elevator settings which are required to trim the machine at the various angles of main plane incidence over the speed range. The full line curve is for flight with engines on and the dotted line for engines off. The coincidence of the two curves shows clearly that the trim of the machine is practically unaffected by the shutting down of all the engines. The vertical distance between the curves A and the remaining chain-dotted curves give the changes of bottom elevator setting required to trim the machine when any one of the engines is stopped.

The problem of the directional control of a multi-engine flying boat with any one engine cut out is similar to that of the aeroplane and need not be considered here.

So far we have considered only the fore and aft trim and control, but where a multi-engine installation is concerned the problem of directional control has to be dealt with so that the machine will fly on a straight course in the event of one engine cutting out.

The first condition to be met is that the aircraft should be able to fly with any one engine cut out. This presupposes a sufficient excess of power under these conditions so that the thrust is at least equal to the drag under the abnormal flight conditions which may result.

Therefore a most important condition is that the directional control shall be sufficient to keep the aircraft flying straight. This is usually accomplished in a multi-engine machine with one wing engine stopped by flying the aircraft slightly yawed with one wing down. In this way a combination of the rudder and aileron is employed to effect the desired result.

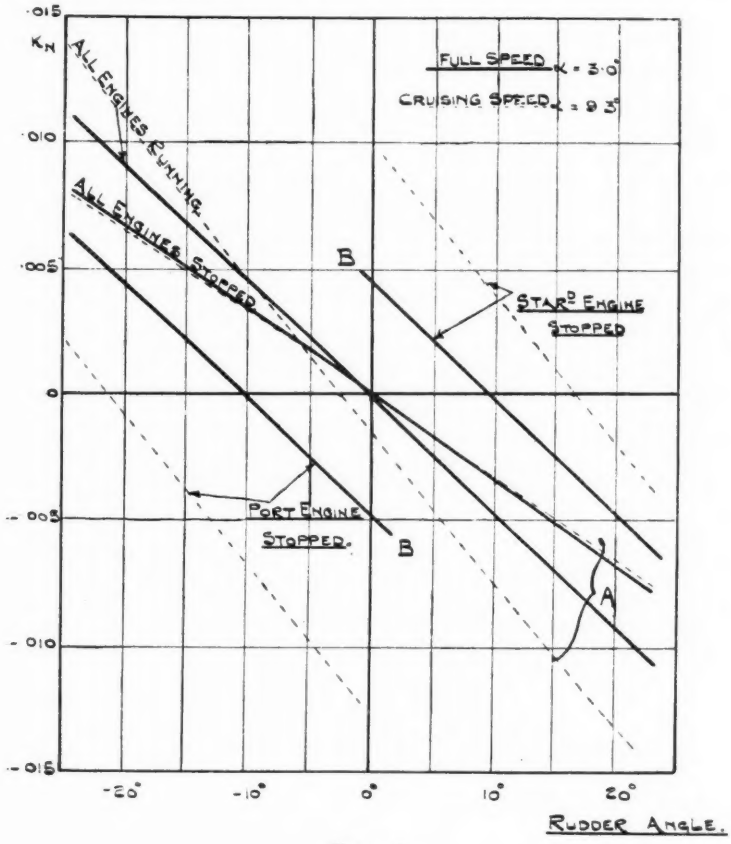


FIG. 26.

Three-Engined Flying Boat.
Effect of Slip Stream on Trim.
Yawing Moment Coefficient at 0° Yaw.
Tail Unit Set at −3.5° to Wings.

The group of curves marked A show that if all engines are stopped at full speed straight flight is maintained without change of rudder control. At cruising speed a movement of 2½ deg. of rudder is required.

Curve B indicates the margin available for turning against engines when one wing engine is cut out.

Fig. 26 shows that in the case of a wing engine cutting out, about 20° of rudder is required at 9° wing incidence to zero yawing moment.

The rudder hinge moments are, however, large, and as the usual balance arrangements were found to be unsatisfactory a servo rudder of the Flettnor type, as developed at R.A.E., was fitted with satisfactory results, control being very light and effective.

Lateral control by ailerons for normal flying speeds presents no difficulty for large machines where the forces are great; effective balance can be obtained either by using the Handley Page backward hinge type of balance or the Bristol Freise. For lateral control at and beyond the stalling point of the aircraft a combination of the slot and aileron control gives, according to the very full information available in the Aeronautical Research Committee's Reports and Memoranda, a solution of the problem.

The figures given above show that the difficulties inherent to the flying boat can be overcome and a satisfactory design evolved which gives adequate control under all conditions.

The conditions to be met with in the design of float seaplanes are somewhat different from those of flying boats. Equally well with the flying boat, the float system must be so designed and placed that the forces due to the motion of the craft on the water do not adversely affect the stability and control. These water forces remain uncontrolled until the higher hydroplaning speeds are reached, hence the float system must be such that any unavoidable instability that occurs between the water-borne and air-borne conditions shall take place at speeds high enough for the air controls to be dominant. Whereas the resistance of an aeroplane in taking off is primarily due to air resistance of the aircraft structure, the seaplane, whether of the float or hull type, has in addition to overcome water forces. This has two principal effects on the design: in the first case the margin of power must be provided so that the machine can pass the hump speed with a reasonable acceleration, and this in itself is a limiting factor as to the total weight per h.p. to be carried. Secondly, it affects the propeller design owing to the necessity for obtaining considerable thrust at low forward speeds with correspondingly bad effect on the top speed of the machine. Further, the necessity of providing larger fin and rudder to balance the large side areas of floats has to be taken into consideration.

Recent tests indicate, however, that the head resistance of a Service aircraft fitted with a float chassis is not appreciably greater than that when fitted with a land chassis, and at sea level the top speed of the seaplane is under five per cent. less than that of the land plane at the same all-up weight.

III. Hull and Float Design

The design of a satisfactory hull calls for a combination of a good many qualities such as seaworthiness, low water resistance, good static stability in all directions, dynamic stability both at taxying and planing speeds, and finally low air resistance when flying.

Just as in the aerodynamic design, wind tunnel research and experiment is essential to obtain accurate figures upon which to base the calculations, so for sea work the model tank originally used by Froude is now universally adopted for model ship testing and the method and apparatus with slight modifications are widely used for model seaplane research work. At the beginning it was necessary only to determine whether Froude's law of corresponding speeds applied to the planing action, in order to make use of the model tests in judging the relative merits of different forms of planing bottoms.

A considerable amount of research work has been done at the N.P.L. and lately by Messrs. Shorts on model floats and hulls in the tank. It has been

definitely established by Mr. Baker as a result of tests carried out on a C.E.1 boat in 1918 that the quantitative results are nearly identical in model and full scale. In these the measured resistance and trimming angle up to the hump speed were similar. The actual qualitative results such as the amount of spray thrown up, etc., are however not similar, as will be seen on the film in slow motion which I will now show of a later type of flying boat.

Model results in the tank do, however, provide quantitative hydrodynamic results and combined with experience in boat construction, hulls can be designed which are very clean both in take-off and alighting.

Seaworthiness of the hull when at rest depends on the size and reserve buoyancy of the hull, the lateral stability when running at taxying speed, on the size of the hull, the shape of the fore body. The most important point in connection with seaworthiness is that there is an ample water clearance for the propeller.

The resistance of the hull to movement through the water depends to a large extent on the water loading of the hull bottom; roughly it can be shown that

> the most efficient hull from the resistance point of view should have a maximum beam measured in inches of about four times the cube root of displacement in lbs.

As all flying boats have a negative metacentric height, lateral stability is obtained either by wing tip floats or some other form of lateral stabiliser. Experience appears to show the most suitable size of wing tip floats may be determined by the following simple rule:—

> The reserve righting moment of a wing tip float in ft.-lbs. should be equal numerically to four times the all-up weight of the aircraft in lbs.

Dynamic stability at taxying speeds is governed by the shape of the fore body sections, which also determine the form of the lines of flow of the water past the hull bottom. If the general convexity of the hull bottom as seen in side elevation is not relieved by the shape of the transverse sections, the flow past the body takes a path which is on the whole convex downwards and a region of low pressure occurs which causes a tendency to nose-dive. The convexity, in side elevation, can however be relieved by giving a concavity to the transverse sections of the bottom so that the general curvature of the flow is on the whole considerably reduced or is even convex upwards, in which case the tendency to dive is eliminated.

The planing bottom should be the concave vee type, which has the great advantage over the straight vee bottom of more effectively keeping down the water thrown out in the form of blisters and spray. In short, the concave vee bottom affords a cleaner and more stable hull or float.

Fig. 27 shows four different types of hull sections at the planing surface: (*a*) flat bottom, (*b*) straight vee, (*c*) slightly concave, and (*d*) concave with acute entry at the keel and flat at the chines.

Type (*a*) may have the highest efficiency, but landing is difficult in even a moderate sea if the bottom is not to be damaged.

Type (*d*) provides a good entry without shock, and while retaining the desirable cushioning effect, sacrifices little, if any, hydroplaning efficiency; also it is a good shape for metal construction when a bar type keel is used.

If the design of the hull is satisfactory for taxying on the water, there next arises the question of the dynamic stability at planing speeds. If this is good there will be an absence of any tendency to what is generally known as "porpoising." The causes of this form of instability are still somewhat obscure, yet from tank tests and full scale experiments considerable progress has been made towards its elimination. This has been accomplished by (*a*) using two steps,

(*b*) keeping the steps well apart with the forward one under the centre of gravity,
(*c*) using a correct shape of hull bottom in way of the steps.

If the steps have been so placed as to prevent or reduce the tendency to instability at planing speeds, and the incidence of the main planes relative to the hull is such that the boat cannot be taken off in a stalled condition under still water conditions, two other requirements should, if possible, be satisfied.

Firstly, the angle between the steps should allow of the boat to be trimmed back to take off, and preferably this attitude should be a natural trim, the moment required from the elevators being insufficient to increase this attitude more than about a degree.

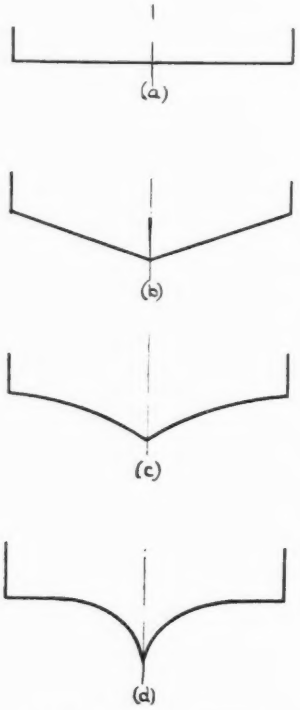


FIG. 27.

Secondly, as the water resistance is a maximum round about the hump speed, and as the hull attitude increases up to this speed, this attitude should be reduced to a minimum, retaining the above requirements. This is of great importance, especially in the case of the heavily loaded seaplane, where there is only a small margin of thrust available for getting over the hump, the total resistance at this speed being the sum of the water and air resistances, the air resistance will be considerable as the plane's incidence may be at a large angle for "taking off."

Against these considerations must be set the difficulty of obtaining adequate propeller clearance if the propeller diameter be large and the engine unit be set low enough in the structure to obtain a reasonable balance of the aerodynamic forces. A compromise has therefore to be made or the power unit split up into a number of smaller units with smaller propellers.

There is the question of obtaining low air drag for the hull. This will depend upon the maximum cross-sectional area and the form, and in the absence of wind tunnel data it is uncertain which has the more influence. For a given beam the hull with a fin top and marked waist line will have less cross-sectional area than one with straight sides, but a higher air and water resistance, and it will obviously be heavier and more costly to build.

Correlated wind and tank tests are not yet available, but would be most valuable.

To keep down the air drag to a minimum the depth of the main step would be as small as other considerations will allow, and the after step should be swept into a vertical transom. This may be done without any sacrifice of hydrodynamic stability.

The float type seaplane is in appearance a normal aeroplane in which the floats take the place of the usual landing chassis. Actually, owing to the different distribution of masses and head resistance it is different structurally and presents a different problem in stability and control.

Three arrangements of float seaplanes are possible:

(*a*) Two main floats which together support the whole weight and provide lateral and longitudinal stability.

(*b*) Two main floats and tail floats, the former supporting nearly the whole weight but being dependent on the latter for longitudinal stability at rest.

(*c*) One central main float supporting the whole weight and providing longitudinal stability, lateral stability being provided by wing tip floats.

System (*a*) provides positive metacentric height, both longitudinal and transverse, while systems (*b*) and (*c*) are dependent on the wing tip floats for lateral stability.

(*a*) *Long Floats*

Experience appears to favour the use of the two long floats and this system is gradually becoming the standard flotation arrangement.

System (*a*) provides positive metacentric height, both longitudinal and transverse, while systems (*b*) and (*c*) are dependent on the wing-tip floats for lateral stability.

The long-float two-step class has a lower hump resistance and up to this hump the trim of the floats is considerably less than in (*b*) and therefore the water clearance of the propeller is not so large. The seaworthy qualities may be not so good under certain conditions as those described under (*b*).

At planing speeds, however, modern development indicates that it is possible to control the attitude of this class of float by the elevator through a range of 6-7° which is sufficient to prevent the nose burrowing in a head-on sea, but not so much as to cause a stall as in (*b*). When drifting backwards this type of float has the characteristic of digging its stern in and rolling over backwards, but it can be definitely cured by a combination of the shape of the after body, the depth of step, curvature of fore body and sufficient reserve buoyancy. This point is important and all floats of this class should be tank-tested for this defect.

(*b*) *Short Floats and Tail Float*

In earlier types the step is formed by the after end of the float and is therefore well aft of the C.G. For this reason the longitudinal stability is bad beyond the hump speed and has to be controlled by the elevator. In later types the stern is extended and intended to form a buoyancy chamber aft of the step, and although this does not improve longitudinal stability at planing speed, it reduces the load on the tail float at rest and taxying speeds. The resistance is high due to the large angle of trim up to the hump speed and also to the large volume of spray from the main floats being thrown against the tail float.

The large trimming angle at low taxying speeds has the advantage of the maximum water clearance of propeller being obtained. At speeds beyond the hump the machine can be controlled to take up large angles of trim. Large angular range of movement is thus available to assist the machine in riding over rough water head-on to sea and prevent any tendency to burrow.

This system of flotation, however, has not sufficient lateral stability without the aid of wing tip floats and is therefore not so stable as the long float system and has the further disadvantage of tending to roll over backwards if drifting astern at speeds as low as five knots.

IV. Hull and Float Construction

The great difficulty of constructing a hull or float sufficiently light, of good aerodynamic and hydrodynamic form, yet capable of withstanding severe water shocks without failure, is not perhaps fully appreciated by those inexperienced in the construction and use of marine aircraft. In spite of this, however, the development of hulls has been both remarkable and rapid and in no other branch of naval construction have such strides been made in developing light hulls for great strength.

The construction of wooden hulls of flying boats up to the end of the war period has been described many times, and it is, therefore, unnecessary to go into details of the earlier types. The predominating type of construction during the war was the braced structure, as exemplified in the F boats. The post-war period developed a radically different type of construction, primarily based on designs evolved by Linton Hope. These hulls are roughly of circular cross sections with the planing bottom built on separately. The construction consists of a large number of small stringers with closely spaced timbers bent round, and at intervals strong circular hoops. This framework is then covered with two thicknesses of planking. This system, with slight modification, is still used in the majority of modern wooden hulls.

The disadvantage of the use of wood for hull construction is the high percentage of water soakage and the difficulty of maintaining the continuity of strength without making the hull excessively heavy.

From a large number of tests carried out, the weight of water absorbed in a few days is roughly 12 per cent. of the bare weight of the hull. This led to the investigation of the possibilities of constructing hulls of metal. At this stage steel was not considered suitable on account of weight and the only alternative was duralumin.

In order to obtain service experience, a duralumin hull was built early in 1925 and this was fitted to the standard F.5 superstructure. Since that date considerable experience has been gained in the detail construction, strength, weight and corrosion problems as compared with wooden construction. Metal construction has been found to present no great difficulties and has possibilities of rapid production by the use of jigs, etc.

As regards the strength of metal hulls, past experience shows that they are at least equivalent in strength to the wooden variety. The weight saved,

according to the type of construction adopted, may be as much as 24 per cent. of the dry weight of the wooden hull, and if soakage is taken into account, 31 per cent.

The latest developments of strip steel construction has led to a consideration of the possibilities of using high tensile stainless steel in the construction of hulls and floats. The advantages of stainless steel, if weight can be kept down, over duralumin, are overwhelming and every endeavour should be made to encourage the use of this material.

Modern Hull Construction

There are two main types of modern metal hull construction, transverse and longitudinal, but the majority of hulls are a combination of the two systems.

In the transverse system the frames are relatively weak but closely spaced and the shell plating and deck forms the principal longitudinal strength. In this system the shell plating takes and distributes most of the load from one part to another and consequently the frames must be so closely spaced that the plate between them cannot buckle or distort.

In the longitudinal system the frames are much deeper and stronger and are spaced at relatively large distances apart, with a large number of heavy stringers, keelson, etc., arranged to run the full length of the hull. This structure is of itself almost sufficient to take and distribute the loads and thus the shell plating can be reduced to a thickness little more than that required to take local water pressure on the bottom.

Floats

The float has been evolved from the three-ply box type construction, which is comparatively easy to build, but is heavy and weak. This type has survived until recently, it has been replaced by a boat-built construction which is lighter and more robust.

As a result of the successful development of metal floats for racing aircraft, these boat-built floats are rapidly becoming obsolete and in place of the box shape, a turtle back, Vee-bottom form, in duralumin is being adopted.

It is interesting to note that it was generally considered that the use of duralumin in the construction of hulls under 80 feet long was impossible, from the point of view of weight economy, and it was even stated publicly so late as 1924.

In the early part of 1925 two pairs of racing floats were built for the same aircraft, one in wood and one in dural of identical size and shape. The weight of the former was 190 lb. each and the latter 165 lb. each. Subsequent mechanical tests showed that the metal floats failed at a load of 13.85 tons and the wooden at 8.75 tons. There are roughly two variations in the type of metal construction now employed :—

(1) A construction that normally follows on the lines of that adapted for hulls, *i.e.*, the transverse frames in one piece with a centre keelson about 6in. deep and side keelsons fitted intercostally between frames in the planing bottom, the skin being strengthened by Z stiffeners.

(2) With one continuous centre keelson to full depth of float. In this case the frames are halved on either side.

Side keelsons are also fitted intercostally between frames in the planing bottom.

In both cases watertight bulkheads are fitted, dividing the floats into a minimum of four compartments.

The Protection of Metal Parts Against Sea Water Corrosion

The rapid development of all-metal machines, the ever-increasing use of lighter sections and the employment of different metals in contact with each other enhances the importance of the problem of corrosion.

Zinc and cadmium coating, no thicker than half a thousandth, gives a considerably longer life to steel than the most satisfactory paint under any conditions met with in seaplanes. No claim is made that either zinc or cadmium coated steel is completely immune from corrosion under all conditions.

On the other hand, steels are now available entirely resistant to corrosion. For the present, however, this super-stainless steel is not likely to be used to any considerable extent on aircraft, for a variety of reasons, primarily on account of cost.

The ordinary non-corrosive steel, halfway house to the super-stainless variety, is now available as a commercial product and in this material there is a possible solution for most of our corrosion problems.

The solution of the problem of the protection of light metals and alloys is generally unsatisfactory as compared with steels. The problem is further complicated in some alloys by intercrystalline corrosion which cannot be detected. The use of paint is of little value; metal protections, such as zinc and cadmium, however, are applicable just as with steel, but more difficult to apply. The alternative non-corrosive light metal does not exist commercially. The last and only alternative at present is the anodic process, which has proved very valuable.

The early hulls were protected by one of the many varieties of paint previously used in marine work.

From experience so far gained the limit of life of duralumin hulls protected by paint has been found to be about 15 months, after which corrosion becomes general.

It may be of interest to mention some comparative tests made on two pairs of racing Dural floats in early 1925. One pair was painted and the other pair was treated by the anodic process. After eighteen months' use the former type showed signs of general corrosion, excessive where water accumulated near the keel and keelsons inside, and local patches of corrosion all over the exterior surface. On the other hand, the anodised float is still in perfect condition, showing no signs of corrosion anywhere.

V. Progress in Flying Boat Design

The improvements in mechanical construction have been reflected in the steady decrease of structure weight as a percentage of the whole which has taken place in post-war designs. In Fig. 29 is shown the structure weight of aircraft exclusive of all internal equipment of hull for weights between 12,000 and 30,000 lb. The progressive improvement in the twin engine type of boat of about 14,000 lb. by improvements in design and by the ultimate change to metal hulls is well shown. With increase in size materials have been used to better advantage and the larger designs show an equal improvement in weight.

Fig. 28 shows the same comparison for hulls alone. The comparisons between the 1918 figures and the 1927 are equally noteworthy.

As the size of the machine increases so does the flying boat become more efficient in structure weight than the land machine. Against the weight of the hull and wing tip floats there is the weight of the fuselage, chassis and tail skid. In Fig. 30 a comparison has been made between the fuselage and chassis weight of various twin engine aeroplanes and that of the hull and wing tip floats for twin engine flying boats, exclusive in both cases of all internal equipment. The

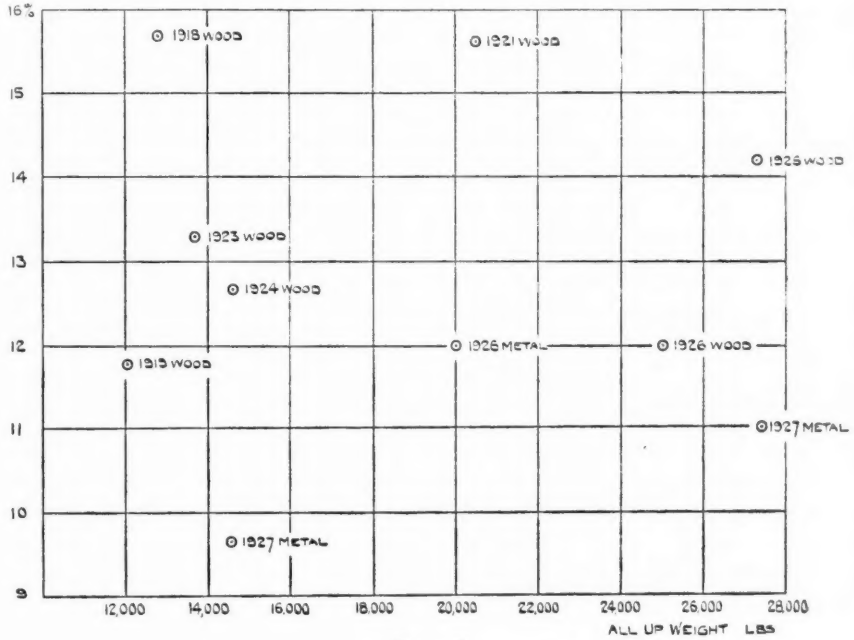


FIG. 28.

Per centage Weight, Bare Hulls (1918-1927).

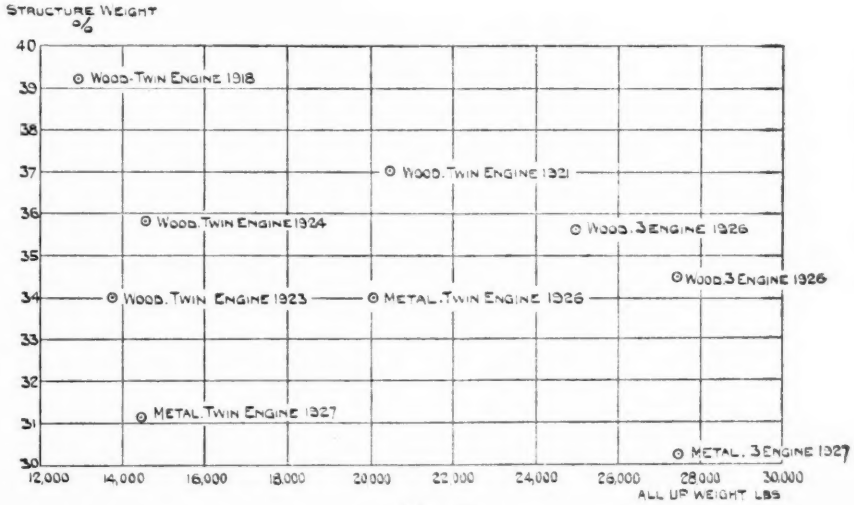


FIG. 29.

Bare Structure Weight Percentage. Flying Boats.

advantage of the flying boat in big sizes is very apparent. Table I. shows the figures in detail from which the curves are made up.

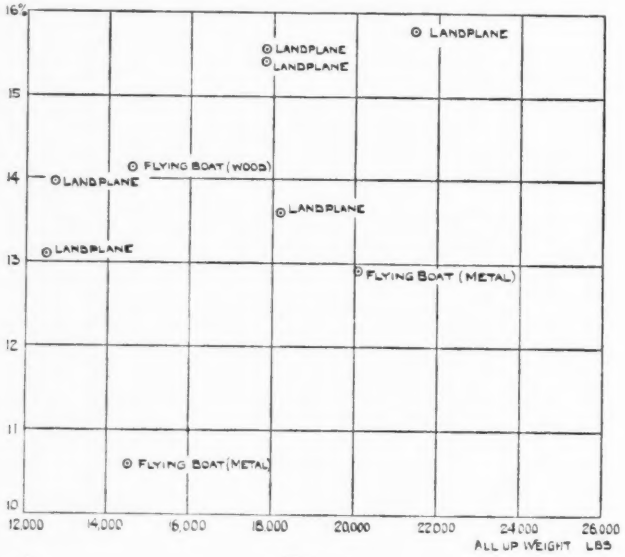


FIG. 30.

Recent Flying Boats and Landplanes (Twin Engine). Hull and Wing Tip Floats v. Fuselage and Undercarriage Percentage Weight.

VI. FUTURE DEVELOPMENT OF THE SEAPLANE

In the foregoing paper I have described in detail the early history of the seaplane and the developments which took place prior to and during the War, as well as the further progress which has been made since 1918 along the slower and surer path which has been possible for research and experiment in peace conditions. In the early stages the difficulties which had to be encountered were much greater than in the case of land machines. A crash very often led to the complete loss of the seaplane, and therefore it was impossible to find the cause of the accident. A further difficulty was the fact that the problem of "getting off" and alighting was not the simple one of dealing with the matter of an aerodrome where the surface was fixed, but conditions varied from day to day, and all these varying conditions of tide, weather and sea had to be met with and the problems arising from them solved quite apart from any question of making the machine a good type of aircraft.

It is only now after the long years of development, the results of which I have described above, that one is able to see the great improvements that have been made in the design of seaplanes and flying boats to withstand the buffeting of rough seas, to take off and alight with a minimum amount of spray, and finally to obtain a structure which was relatively as light as in the aeroplane of corresponding weight.

TABLE I.

Bare Hull or Fuselage and Bare Structure Weight.

	Boat	Sum of Fuselage (bare), Chassis and Tail Skid or Hull (bare) and Wing Tip Floats. %	Engine.	Total Weight. lbs.	Wing Structure lbs.	Tail Unit. lbs.	Wings and Tail. %	Fuselage and Tail Skid or Hull lbs.	Fuselage and Tail Skid or Hull %	Chassis and Tail Skid or Wing Tip Floats. lbs.	Chassis and Tail Skid or Wing Tip Floats. %	Power Plant and Load, including Petrol and Oil. lbs.	lbs./sq. ft.	lbs./H.P.	Structure Weight Ratio (bare). %
1		16.6	Rolls Eagle VIII.	12000	2613	345	22.9	2004	15.5	144	1.1	7690	9.3	18.2	39.5
2		13.0		12055	1861	384	18.6	1420	11.77	147	1.22	5372	9.4	17.0	31.6
3															
4		16.45	Rolls Condor (2)	20500	3733	480	20.5	3209	15.04	162	.79	12479	10.65	15.3	37.0
5		14.5	Lions (2)	13790	2305	385	19.5	1831	13.3	161	1.17	5606	10.8	14.7	34.0
6	Wood	14.15	Lions (2)	14600	2767	396	21.7	1850	12.67	216	1.48	8347	10.1	15.5	35.8
	Metal	10.6		14600	2595		20.5	1410	9.6	144	.98	8347	9.67	14.9	31.1
7	Wood	15.7	Condors (3)	27428	4620	538	18.8	3881	14.2	432	1.57	16527	11.15	13.65	34.5
	Metal	12.2		27428	4360		17.9	3035	11.0	332	1.2	17733	10.7	13.1	30.2
8		13.2	Condors (3)	25000	4862	740	22.4	3010	12.0	300	1.2	13977	—	12.44	35.6
9		12.9	Condors (2)	20030	3738	474	21.0	2400	12.0	186	.93	11642	11.35	14.6	33.9
10		14.17	Jupiters (3)	19700	3060	465	22.4	2605	13.22	186	.94	11080	—		36.6
	Land														
1		15.4	Lions (2)	17810	3836	249	22.9	2108	11.8	633	3.56	10196	8.15	19.0	38.3
2		13.0	Lions (2)	12544	2461	244	21.6	1117	8.9	521	4.15	7884	9.45	13.3	34.6
3		13.6	Lions (2)	18145	4207	273	24.7	1796	9.9	666	3.67	10624	8.38	19.3	38.3
4		15.55	Jaguar (3)	17828	3256	308	20.5	2049	11.5	722	4.05	10540	9.42	15.6	36.0
5		13.07	Lions (2)	12739	2311	166	19.4	1197	9.4	583	4.57	8230	8.58	13.4	33.4
6		15.77	Condor (2)	21440	3980	532	21.0	2279	10.62	1104	5.15	13240	10.34	16.0	36.8

Experience has shown, and it is indeed obvious, that the *small* flying boat with the single engine fitted in the wing structure can never be aerodynamically efficient, and in consequence of this the development of the large multi-engine machine has become a necessity. As in the case of ships, the larger the vessel the more seaworthy, and this circumstance will inevitably lead to a great increase in size. The many complicated and controversial problems with regard to the design and handling under operational conditions of large flying boats can only be satisfactorily solved by gradual development. If this development were limited entirely to increase in size without alteration of geometrical form or of the materials used in construction, a limit of useful size might almost be within sight.

The experience already obtained in the design and construction of large flying boats and aeroplanes has shown that still larger machines can be built than those already constructed without that increase in structure weight which theoretical considerations show must ultimately impose a limit on size.

There is one feature in the development of big machines, viz., the size and disposition of the power units and propellers that will have a determining influence upon the future size of aircraft. As the flying boat increases in size so there should be obtained an increased water clearance for propeller, and therefore a greater seaworthiness in rough weather. It is a matter of actual experience that in the larger boats which have been built to-day, the water clearance for the propeller is not greater than in the smaller ones, owing to the employment of a similar number but larger size power units which call for an increase in size of propeller with increased size of boat. If full advantage is to be taken of the greater seaworthiness of the large boat the power units must be increased in number and not in size. The same diameter propeller as in the smaller craft will then be used and the propeller clearance greatly increased. Otherwise the seaworthiness of the large flying boat will be more apparent than real.

New developments to-day in the use of variable wings, in the employment of higher loadings and full application of metal construction will all have their cumulative effect in making design more efficient. One would hardly expect very startling improvements in the wing structure weight owing to the small proportion that the wing structure weight bears to the whole in a successful modern design. Opportunities do, however, exist to-day in the improvement of the design of the supplementary and equipment parts of the aircraft, which are not properly speaking part of the structure weight. For these materials of high tensile strength are not necessary, and if new alloys can be obtained of light weight, their use would greatly help to reduce the weight of the aircraft as a whole.

As great, or even greater, possibilities lie ahead of us in the use of higher compression ratios in the engine and consequent fuel economy and geared variable pitch propellers, increased efficiency of the power unit and reduced fuel consumption offer bigger scope to-day for seaplane improvement than almost any other channel. All of these improvements will make still more apparent the improved conditions which the large flying boat offers, owing to its increased efficiency and increased range.

It is not only for military and naval purposes that we in the British Empire have to consider the large seacraft. For commercial work the seaplane or flying boat has qualities to offer to the travelling public greater than the corresponding land machine once it is proved to the public that travel by air over the sea is not attended (except as a very remote possibility) by the chance of forced alighting and shipwreck. The aeroplane, where a large volume of passenger traffic is available, has to compete with railway services with speeds of anything up to 60 m.p.h. The seaplane or flying boat rarely has to consider anything more than

a 15 or 17 knots competitor. It is a fact to-day that the aeroplane only offers a real advantage as a competitive means of transport to land and sea facilities where narrow straits of water have to be crossed which greatly decrease the normal rate of travel. An excellent illustration of this is the London-Paris service.

For this country development of the seaplane is of paramount importance. We do not possess aerodromes or alighting harbours closer than Gibraltar or Malta. These provide much better facilities for flying boats than for land machines. All along our present trade routes where coaling stations exist for the Navy, seaplane bases have been or could be formed. We have, therefore, with little expense, all the harbour organisation available for the development of seaplane or flying boat routes, and these lie at points already familiar to the traveller by sea and are available at little cost to this country. Wherever we look on Imperial air routes, the situation is marked out for development by flying boats. The routes were opened up by steamships and it is only now by the use of the higher speed form of transport by air that speeds along these routes can be accelerated.

We in this country who, by tradition, are more intimately associated with the design of craft for sea operation than any other country and have led the world in shipbuilding and ship operation, have designers and manufacturers who equally well pioneered the development of seaplane and flying boat design. The results which I have given indicate only what has been done. They equally well show the possibility that lies ahead if full scope is given to our latent potentialities for design of commercial flying boats.

Appendix I.

Stability Problems of Flying Boats

The metacentric method of dealing with stability problems is suitable for use in the calculation of the required buoyancy of wing-tip floats and their effect on the stability of flying boat hulls. Unless some stabilising device is fitted, either in the form of extensions to the hull or as separate floats, all flying boats are unstable when in an upright condition on the water. Some boats with excessive beam for their size have been found to be nearly stable.

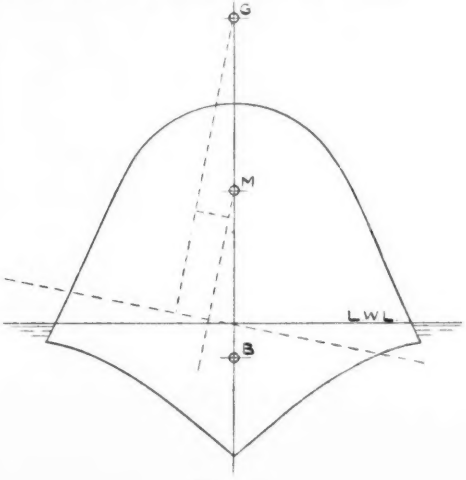


Fig. A.

As will be seen on reference to any book on naval architecture, the height of the metacentre above the centre of buoyancy is found by the simple formula of I/V, where I is the moment of inertia of the waterplane and V the volume of the underwater body. If the centre of gravity is above this point, then the boat is unstable.

In Fig. (A) let B be the centre of buoyancy, M the metacentre and G the centre of gravity. It will be seen that if the boat is slightly inclined, G which is acting downwards and B which is acting upwards are not in the same line and thus there is a couple tending in this case to heel the boat to a larger angle. The measure of this force for small angles of inclination, such as reached by flying boats, is given by $W \times GM \times \sin$. If a curve of upsetting moments based on this formula is plotted it is to all intents a straight line as it is not necessary to go to greater angles than 10° or 12°. This upsetting moment has to be balanced by the righting moment of the wing-tip float.

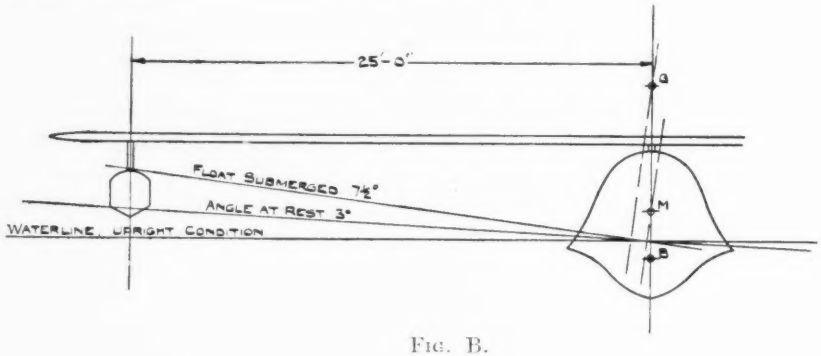


FIG. B.

In Fig. (B) is given a diagram representing, say, a boat of 20,000 lbs. displacement, which is shown at rest on the water at an angle of 3°. GM is shown on the figure as being $-6'$, the upsetting moment is therefore

$$20000 \times 6 \times \sin 3° = 6280 \text{ ft. lbs.}$$

The wing-tip float is shown as being 25ft. out from the centre line and the righting moment is therefore $B \times L \times \cos$ where B is the buoyancy of the wing-tip float to the waterline shown and L the distance from the centre line of the float to the centre line of the boat. A simple calculation will show that B in this case must be 251 lbs. to balance the upsetting force. If, therefore, we measure up the buoyancy of the float to where waterlines at various angles of inclination cross it and plot off on the curve previously mentioned the righting moments so calculated, the point where the curves cross will be at the angle where the boat will lie at rest and the distance between the two curves beyond this point will represent the available reserve moment to counteract the effect of any external forces, such as wind, tending to upset the boat. Very little error will be introduced by making the waterlines at the various angles cross the waterline at rest at the centre line of the boat.

It will be seen from the above that the earlier the buoyancy of the floats comes into play, the smaller the upsetting forces are, but as the water forces on the hull when taxying keep the aircraft upright when the speed exceeds about 20 knots it is better to keep the floats out of water at the higher speeds and so decrease the resistance. The blows on the floats when driven at high speed through waves tend to put heavy loads on the floats and wing structure, and for this reason the floats are better kept high up. The actual distance is thus a

compromise between the two conflicting requirements. The transverse angle at which the majority of boats lie at rest on the water averages about 3°.

The first item to be considered is the moment of inertia of the waterplane, it being presumed that the load waterline has been calculated. The moment of inertia can be calculated in any of the usual ways, but the general practice in naval architecture is to use the half-breadths at the various sections and put them through one of Simpson's rules. This is a very simple calculation and an example is given in Fig. (C) for the plane of flotation shown in Fig. (D). As

No.	½ Ord.	Cubes.	S.M.	Functions.
1	—	—	½	—
1½	1.7	4.91	2	9.82
2	2.7	19.68	1	19.68
2½	3.4	39.30	2	78.60
3	3.8	54.87	1½	82.30
4	4.2	74.09	4	296.36
5	4.2	74.09	2	148.18
6	4.3	79.51	4	318.04
7	4.3	79.51	2	159.02
8	4.2	74.09	4	296.36
9	4.0	64.00	2	128.00
10	3.8	54.87	4	219.48
11	3.6	46.65	2	93.30
12	3.4	39.30	4	157.20
13	3.1	29.79	2	59.58
14	2.8	21.95	4	87.80
15	2.4	13.82	1½	20.73
15½	2.1	9.26	2	18.52
16	1.7	4.91	1	4.91
16½	1.2	1.73	2	3.46
17	—	—	½	—
				2201.34

Volume = 20000/64 = 312 cub. ft.
M.I. = 2201.3 × 2.5/3 × 2/3 = 1223.
B.M. = 1223 ÷ 312 = 3.92′.

FIG. C.

cubes of the ordinates are used it is better to put in as many ordinates as possible, or in any case to introduce some ordinates at half-spacings at the end of the waterplane. The half-ordinates are cubed, multiplied by the appropriate Simpson's multiplier and the products summed up. The sum is then multiplied by one-third of the spacing of the ordinates and by two-thirds to take into account both sides. The result is then divided by the volume of the immersed body, the resulting figure being the height of the metacentre above the centre of buoyancy.

The vertical position of the centre of buoyancy has now to be found. If, when the displacement is being checked, an integrator is used and the moments read off at the same time as the areas, the work is simplified. Alternatively, a displacement curve of the hull may be drawn (similar to Fig. (E)), then the area of the curve divided by the ordinate at the waterline gives the distance of the centre of buoyancy below that waterline.

It is presumed that the approximate position of the centre of gravity is known at an early stage of the design and the relative positions of *B*, *M* and *G* can now be plotted off (Fig. A). *GM* being now known a curve of upsetting moments can be plotted, as in Fig. (F).

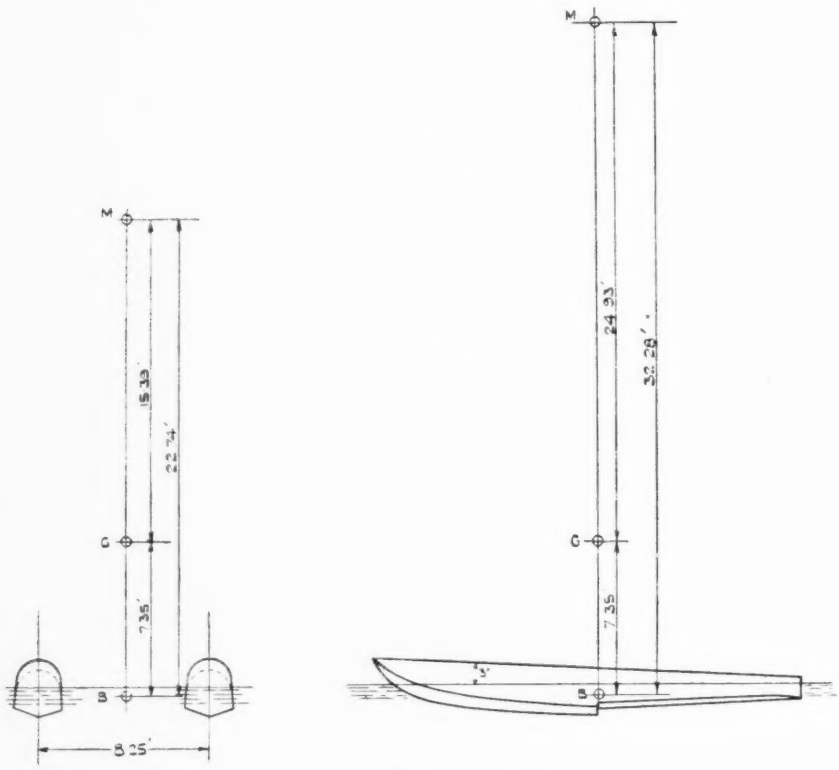

M
15.39'
22.74'
G
7.35'
B
8.25'
M
24.93'
32.28'
G
7.35'
B

FIG. D.

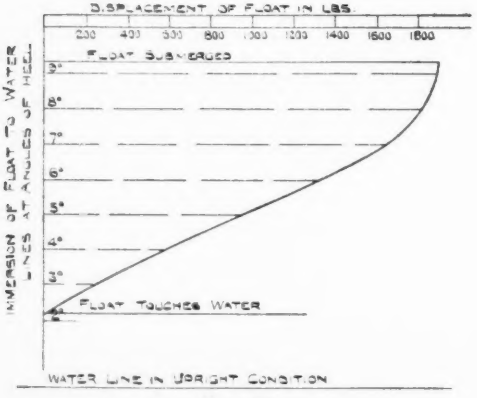

DISPLACEMENT OF FLOAT IN LBS.
200 400 600 800 1000 1200 1400 1600 1800
FLOAT SUBMERGED
9°
8°
7°
6°
5°
4°
3°
FLOAT TOUCHES WATER
WATER LINE IN UPRIGHT CONDITION
IMMERSION OF FLOAT TO WATER LINES AT ANGLES OF HEEL

FIG. E.

The position where the lines of various angles of inclination cross the centre line of the wing-tip float are now drawn on the body plan of that float, and the displacement to each of these lines is calculated, taking care that the fore and aft run of the waterlines on the float are in keeping with those of the main hull. The curve of righting moments due to the buoyancy of the float can now be plotted, the lower starting point being obviously at the angle where the float just touches the water and the top of the curve flattening out at the angle at which it is completely submerged. (See Fig. (E) for displacement curve of float used as an example.)

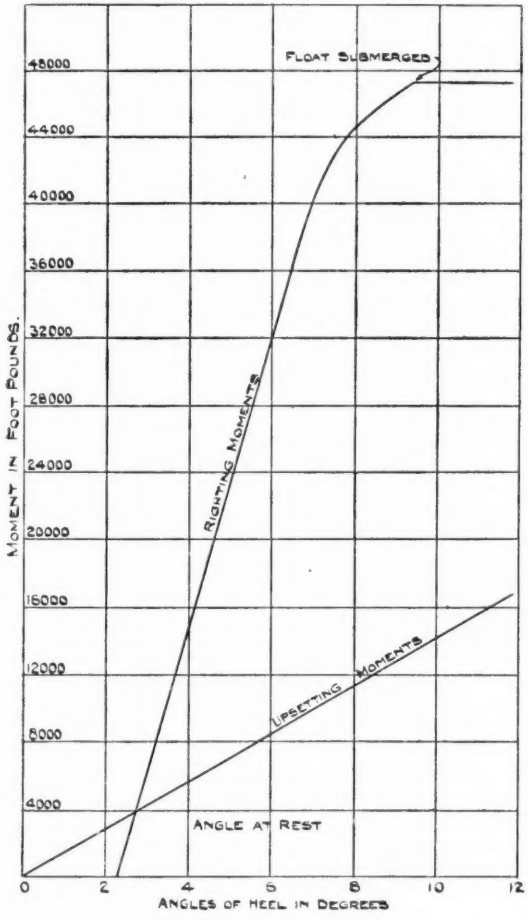


FIG. F.

In Fig. (F) which is plotted from the figures used as an example, and which are not based on any boat in particular, it will be seen that the reserve righting moment at 10° heel, when the float is submerged, is 33,000 ft. lbs. As it is stated in the body of the paper, the reserve moment should be of the order of

4 ft. lbs. for every lb. of displacement, it will be seen that the size of float used in the example would be too small and would either have to be increased in size, moved further out, or a combination of both variations could be used.

APPENDIX II.

Stability Problems of Float Seaplanes

The amount of stability required in a twin-float seaplane to enable it to withstand the capsizing moments caused by wind, taxying in rough water, leakage of water into a float, or from any other cause that affects the stability, has received very little attention from designers in the past, and the stability of comparable seaplanes by various designers has varied quite 100 per cent. Floats have sometimes been put on a machine so as to suit the structure of the fuselage and without any regard to the stability of the system or the best position of the C.G. relative to the floats. What would otherwise be quite good floats have been spoilt by being placed too far forward or aft, or too close together for the machine.

Reserve buoyancy, too, must be borne in mind in stability problems, as reserve buoyancy gives similar qualities to the float system that freeboard does to a ship.

The position of the centre of gravity of a machine is known, at least approximately, in an early stage of the design, but to know how stable transversely and longitudinally the seaplane will be, it is necessary to know the position of another centre. This is termed the metacentre, *i.e.*, the point or centre above which the centre of gravity cannot rise without the vessel becoming unstable. The metacentre is not a fixed point, save for small angles of heel.

Dealing with the transverse metacentre first, in the upright condition the position of the metacentre is easily found relative to the centre of buoyancy. The moment of inertia of the waterplane divided by the volume of displacement will give the vertical height of M above B. The measure of stability is $G-M$ which is known as soon as M is found. The weight of the machine is acting downwards through G and the buoyancy is acting upwards through M, thus if M is above G the machine has some degree of positive stability. For small angles—varying with the shape of the floats—M may be taken as constant and the righting arm is obviously $GM \times \sin$, which multiplied by the weight gives the righting moment. For large angles, say 6° or over, it will be necessary to make separate calculations for the position of M. In the case of the longitudinal metacentre the shape of the waterplane varies rapidly when trimmed, shortening up quickly when the sternpost is submerged, so that M can only be taken as constant for two or three degrees.

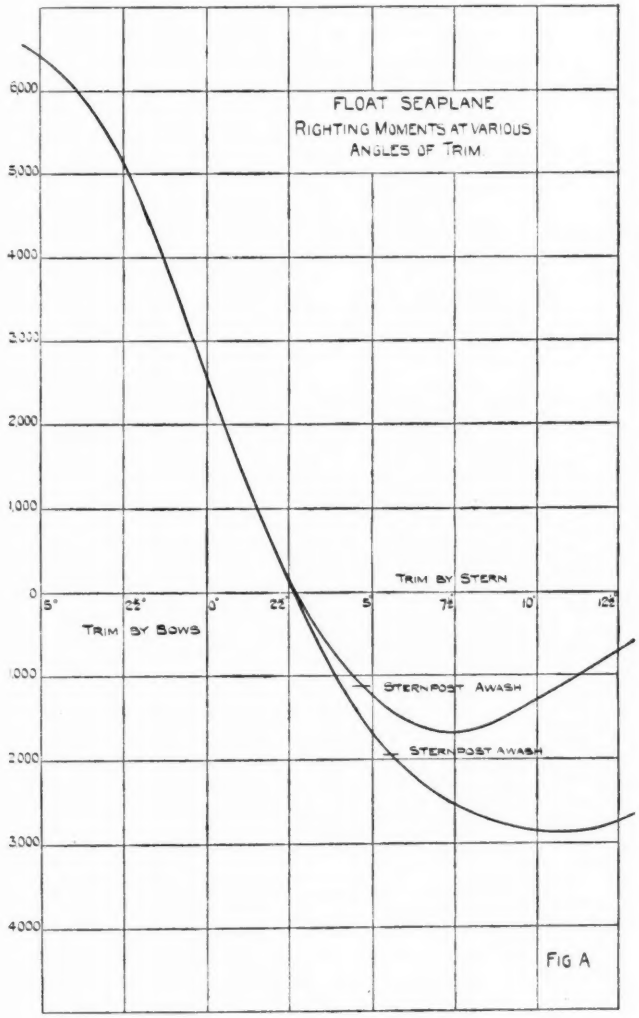
The majority of float seaplanes have a small margin of longitudinal stability when trimmed aft. They are, however, much safer when taxying as the vertical component of the water forces is acting some distance behind the centre of gravity and so making them stable even to twice the angle at which they would roll over backward when at rest.

The accompanying figure (A) shows the righting moments of a small seaplane when trimmed by the bows or stern and the dotted line shows the additional stability gained by a small increase of buoyancy aft. As this buoyancy was added on the top it would have no effect on the running angles of the floats, these being governed by the water forces on the bottom, save at low speeds where there would be a slight difference,

For most seaplanes it is only necessary to calculate the position of M for the at-rest position, as, if this is satisfactory when compared with the known behaviour of other seaplanes, and the reserve buoyancy is up to the standard,

the range of stability is also satisfactory. It is advisable to make an extra calculation or two on the longitudinal stability when trimmed aft.

The simplest way of finding the position of M is to make use of the equidistant sections used in drawing out the lines of the floats. After the load



waterline has been calculated the heights of it at each section are transferred to the body plan (this would be automatically done in checking the displacement and position of the centre of buoyancy) and a plan of the half-breadths at the waterline laid off. If the waterline is the same length as the float, or ends definitely at a section, the ordinates at the sections can be used, but if the

waterline ends between sections the length must be specially subdivided to give a suitable number of equally spaced ordinates to suit Simpson's rules. These ordinates are then used for finding the moment of inertia of the waterplane area about the longitudinal axis and also a transverse axis through the mid-ordinate, the latter being afterwards corrected to give the moment of inertia about the neutral axis, the tabulated figures supplying the correction. It may be thought that longitudinal ordinates would be more suitable for use in this part of the calculations, but a little thought will show that these lines would be difficult to measure with sufficient accuracy owing to the small angle at which they would cut the boundary of the area, and also that the end ordinates would be a tangent to the curve, which is to be avoided in using Simpson's rules. As it is, where accuracy is required, use must be made of subdivided spacing of the end ordinates, as cubes make the curve very abrupt.

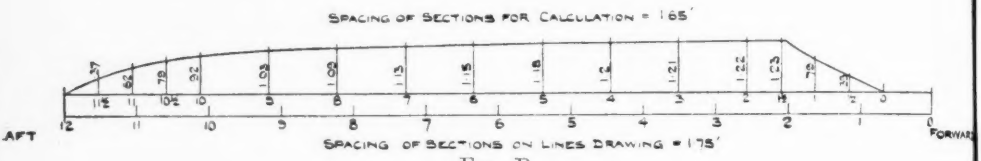


FIG. B.

A specimen drawing of the plan form of the line of flotation of a float is given in Fig. (B). In this case the waterline did not end at any section on the lines drawing so new ordinates were drawn in as shown. The whole of the calculation is shown in Fig. (C) and a brief statement of the various operations is all that is necessary.

Column (1) is the number of the ordinates.

Column (2) is the half-breadth of the float at each ordinate.

Column (3) is the appropriate Simpson's multiplier.

Column (4) are the products of (2) and (3). The sum of this column multiplied by one-third the spacing of the ordinates and by two for both sides gives the area of the plane of flotation.

Column (5) is the number of the ordinate from the centre one, *i.e.*, for moments.

Column (6) is the product of columns (4) and (5) and is obviously the moment of the area or length about the centre ordinate. These are added up separately for both sides of the mid-ordinate and the smaller subtracted from the larger. Then the difference multiplied by the spacing of the ordinates in feet (if feet are used) and divided by the total of column (4) will give the distance of the centre of gravity (centroid) from the mid-ordinate towards the end with the largest product.

Column (7) are the levers in (5) repeated.

Column (8) is the product of columns (6) and (7) and is thus equivalent to multiplying the ordinates by the square of their distance from the mid-ordinate. This column is totalled right up and dealt with thus :—

For the Simpson's rule used it is multiplied by one-third the spacing of the ordinates, by the spacing squared, and by two for both sides.

This, evidently, is the moment of inertia about the mid-ordinate, but it is required to find it relative to the axis through the centre of gravity. The distance between these two points has already been found and also the area of the waterplane. It is then necessary to multiply the area by the distance squared and deduct the product from the sum found above.

Floats 21′ O.A. Spacing of Sections = 1.75′.
Centre of Gravity = 6′ above Top of Float.
Weight 3800 lbs.
Step 7″ aft of ⌑.
Spacing of Floats 8′ 3″.
Angle of L.W.L. to Datum 3°.
Reserve Buoyancy 90%.
Thrust Line Parallel to Datum.
Wing Chord + 3° to Datum.

	1 Sec. No.	2 ½ Brdth.	3 SM.	4 F for Area.	5 Lever.	6 Moment Product.	7 M.I. Mult.	8 M.I. Product.	9 ½ B³	10 Cubes × SM.
F	0	.0	½	0	—	—	—	—	.0	.0
	½	.39	2	.78	5½	4.29	5½	23.60	.06	.12
	1	.79	1	.79	5	3.95	5	19.75	.49	.49
	1½	1.23	2	2.46	4½	11.07	4½	49.81	1.86	3.72
	2	1.22	1½	1.83	4	7.32	4	29.28	1.81	2.71
	3	1.21	4	4.84	3	14.52	3	43.56	1.77	7.08
	4	1.2	2	2.4	2	4.8	2	9.6	1.73	3.46
	5	1.18	4	4.72	1	4.72	1	4.72	1.64	6.56
	6	1.15	2	2.30	—	50.67	—	—	1.52	3.04
	7	1.13	4	4.52	1	4.52	1	4.52	1.44	5.76
	8	1.09	2	2.18	2	4.36	2	8.72	1.29	2.58
	9	1.03	4	4.12	3	12.36	3	37.08	1.09	4.36
	10	.92	1½	1.38	4	5.52	4	22.08	.78	1.17
	10½	.79	2	1.58	4½	7.11	4½	31.99	.49	.98
	11	.62	1	.62	5	3.10	5	15.50	.24	.24
	11½	.37	2	.74	5½	4.07	5½	22.38	.05	.10
A	12	.0	½	.0	—	41.04	—	—	—	—
				35.26		9.63		322.59		42.37

FIG. C.

Disp. in cub. ft. = 3800 ÷ 64 = 59.37

$$\text{Area} = \frac{35.26 \times 1.65 \times 2}{3} = 38.78 \text{ sq. ft.}$$

$$\text{C.G.} = \frac{9.63 \times 1.65}{35.26} = .45' \text{ forward of 6}$$

(1.03′ forward of step)

$$\text{Longitudinal M.I.} = \frac{322.59 \times 1.65}{3} \times 1.65^2 \times 2 = 966.04$$

C.G. Correction = $38.78 \times .45^2 \times -7.85$

$$\text{Longitudinal B.M.} = \frac{(966.04 - 7.85) \times 2}{59.37} = 32.28'$$

$$\text{Transverse M.I. of One Float} = \frac{42.37 \times 2 \times 1.65}{3 \times 3} = 15.54$$

$$\text{Transverse B.M.} = \frac{(38.78 \times 4.125^2 + 15.54) \times 2}{59.37} = 22.74'$$

The moment of inertia about a transverse axis has now been found, and multiplying this by two for both floats and dividing the sum by the volume of displacement in cubic feet, the result will be the height of *M* above *B*, that is, the position of the longitudinal metacentre.

Column (9) gives the cubes of the half-ordinates.

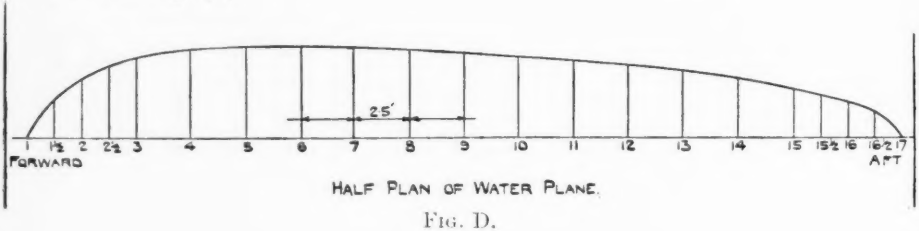
Column (10) gives the cubes multiplied by the appropriate Simpson's multiplier (Column 3).

The sum of these functions is, as usual, multiplied by one-third the spacing of the ordinates and by two-thirds, the two being for both sides and the one-third being the usual one-third of the area of a curve of cubes.

The moment of inertia of one float about its longitudinal axis has now been found and there remains only the question of the two floats spaced a given distance apart. The area of one float is already known, and this, multiplied by the square of the half-spacing of the floats, plus the moment of inertia of the float itself, then the sum multiplied by two for both sides, gives the total moment of inertia of the system. Nothing now remains but to divide this product by the underwater volume, as before, and the height of the transverse metacentre above the centre of buoyancy is thus obtained.

The one thing not already found is the vertical position of the centre of buoyancy from which to set off the position of *M*. This may be found in various ways, the simplest being the use of an integrator when measuring up the sections for displacement. Another way is to measure up and set off the displacement to a series of waterlines, thus giving a displacement curve from which the distance of the C.B. below the waterline is easily obtained by dividing the area of the curve to the waterline by the length of the ordinate at the waterline. Alternatively, the average distance of the C.B. below the load waterline of a large number of floats is .2 of the maximum draught of the float to the same waterline.

The results now arrived at may all be plotted off in a graphical manner as shown in Fig. (D).



HALF PLAN OF WATER PLANE.

FIG. D.

DISCUSSION

Colonel the MASTER OF SEMPILL: The lecturer has had a very lengthy experience of marine light aircraft, and has, for a number of years, been the head of that section in the Air Ministry which deals with this design and the many technical problems arising from their use. That seaplanes and flying boats have been grossly neglected is common knowledge, in fact the period following the war was governed by the supreme official policy which openly affirmed that seaplanes were no good. For some years nothing was done to encourage the production of new types, or even to find out the absolute limitations of existing types. The carrying out, or even the attempt to carry out long cruises with the war designs would have produced results of great value. The future Air Routes of the Empire would have been pioneered and technical information of the utmost value produced.

Major Penny has lived through this barren period and the fact that the official view has, at last, changed for the better, must be largely due to his energetic persistence. There is a lot of leeway to make up as may be gauged by the great success that flying boats, designed and operated by other nationals, have met with.

Even to-day official opinion is not really alive as to what it can do. The serious proposal to pack and ship certain flying boats some 5,000 miles, and then to erect them and commence a cruise of which you will have read in the daily Press, is sufficient illustration of this point.

On the side of commercial aviation the position is even more unsatisfactory, as we have no commercial flying boat in existence, and, in consequence, no service. I, for one, am a believer in the flying boat as the mainstay of future commercial service, for our Empire in particular. The flying boat has several great advantages over the aeroplane, and one must not forget that this globe is mostly covered with water.

We pride ourselves on being a seafaring people, and that the Mercantile Marine is our mainstay, and yet we have no seagoing Air Service although there are a number in regular, and very successful, operation in Europe alone.

I will now ask for Captain Cave-Browne-Cave, the Deputy Director of Technical Development, to open the discussion.

Group Captain H. M. CAVE-BROWNE-CAVE: The table of structure weight percentages is very interesting, but I am inclined to mistrust tables of this nature, because they do not include all the factors which have a considerable effect on structure weight percentage; for example the duty for which the aircraft was designed, the load factors and the stalling speed. If we could accept the evidence of this table that seaplanes of quite moderate size are already as good or better than the corresponding landplane, then it would be reasonable to expect the large seaplane to be considerably ahead of the large landplane in a few years' time, because fewer have been built, and consequently they are in a less developed state where more rapid progress is possible.

I do not agree with the lecturer's view that provided that water clearance of the airscrews is increased by increasing the number instead of the power of the engines—then large flying boats will be necessarily more seaworthy than small ones. Seaworthiness depends upon a great many factors and the water clearance of the airscrews is only one of them. It is certainly very important, but the comparison must be between clearances from the place where the waves and spray are present in sufficient quantity to damage the airscrews, if sucked into them, and not only the clearances from the tips of the airscrews to the water level at rest. Other things being equal, the larger boat will make larger waves as it ploughs through the water, and it is quite possible for the larger boat with the larger water clearance at rest, to have a smaller effective airscrew clearance than the smaller boat, when attempting to "get off." Another factor affecting water clearance of the airscrews during "get off" is the controllability of the boat. Large flying boats are slower on control than small ones, and consequently the pilot is less able to protect his airscrews by altering the trim of the boat to meet a sudden emergency during the critical period. I think it is generally agreed that, with equal knowledge and ability in design, as the size of the flying boat increases so will the structure weight percentage increase, unless the designer takes drastic steps to keep it down. The increased size makes it possible to use greater refinement of design without loss of robustness in the smaller parts, but this by itself does not appear to be enough, and there is a strong tendency to keep the structure weight down to a reasonable figure by reducing the reserve of strength, *i.e.*, the load factor, and reducing the wing area for a given load, *i.e.*, increasing the landing speed. The larger boat consequently tends to be slower on control, have a smaller reserve of strength and

a higher landing speed than the smaller boat, and these factors do not, I think, lead to increased seaworthiness under equal sea conditions. Further, the smaller boat will frequently be able to use harbours and sheltered waters for " getting off " and " landing " when the larger and less handy boat will have to go outside.

I think there is a great future for flying boats of moderate size, and that increased seaworthiness should be sought by getting more experience with boats of the size we have now, rather than by increasing the size.

Major Penny could not deal with all the important considerations affecting flying boat development in the time at his disposal, and there are two I should like to mention. One is that in designing the shape of the hull for good " get off " and low air resistance, some consideration should be given to its suitability for beaching, so that, if this operation becomes necessary, it will be possible to carry it out safely on a suitable beach without the danger of the boat rolling over and damaging the wing tips or floats before they can be shored up. The other is the necessity for providing, during the design stage, for the comfort of the crew when living on board, and facilities for changing engines, refuelling and maintenance when the flying boat is afloat. Unless this is done during the design stage these essential requirements add unnecessary weight, and until they are provided for, the use of the boat is restricted and costly. Some recent measurements of the strains in a flying boat hull showed that these were greater when the boat was being handled on the beach trolley than they were when moored out, " getting off " or " landing " under adverse conditions, and I think it is important to reduce the need for getting boats ashore as much as possible

Major J. D. RENNIE: My own experience leads me to agree, with the exception of a few points, with the views expressed by the author.

With regard to the comparison of the weights of duralumin and wood hulls, it should be remembered that the latter hulls are fitted with double bottoms, so the comparison is only fair provided it is accepted that a double bottom is unnecessary in a duralumin hull. In my opinion it can be dispensed with if the planing bottom is made sufficiently strong to withstand the conditions, apart from water forces, which may lead to a failure. On the other hand, the light alloy hull can be designed for less weight, of greater local strength, and in production cheaper than the corresponding wood hull.

Communicated.—The author states that the most important point in connection with seaworthiness is that there is ample water clearance for the propeller. The difficulty is to define what clearance is ample, and under what sea conditions. Obviously the propeller should be clear of solid water and spray under all conditions, but this may tend to an uneconomical structure and to aerodynamic difficulties, which may, however, not be serious and lead to an acceptable design, if this condition must be fulfilled.

Sea conditions prevail in which some ships, depending upon their size and seaworthy qualities, are unable to put out to sea, and if at sea have to run for shelter. So with the flying boat, sea conditions can prevail in which a take off is either dangerous or impossible, depending again upon size and seaworthy qualities.

Given, then, the worst possible sea and air conditions under which the boat can take off, risk of damage to a propeller and possibly an engine should not prevent operations under these conditions, if such may be avoided in the design, either by increased propeller, water clearance, by engine position, or by the form and dimensions of the hull forebody.

I believe statistics of such damage under these conditions will show that with the designs as used in this country such an occurrence is rare.

Wing Commander R. B. MAYCOCK: I agree with Group Captain Cave-Browne-Cave that increased seaworthiness will not be obtained entirely by enlarging the

hulls of flying boats, in fact I consider there is a definite proportion between the size of the flying boat, the speed of take off, and the state of the sea, at which the flying boat will be found to be the most seaworthy, until of course dimensions beyond anything we can reasonably foresee at present have been reached. Commander Tower gave an instance of this in landing off the Azores in one of the N.C. type in a mountainous sea and taxying safely to shelter. It is probable that some of the larger types of to-day with two-bladed propellers, and consequently low propeller clearance could not do this.

I feel sure we have a lot more to find out about this before we can achieve much advance in seaworthiness.

The Southampton is the standard type in service to-day and in spite of its small size is the most seaworthy of those I have had experience with.

With regard to propeller clearance, even a few more inches would make the Southampton more seaworthy than it is now. Some little time ago off Cromer a Southampton took off in what the pilot estimated to be an eight-foot sea and although both propellers and the aerostructure were damaged, it was possible to fly back to the base. In this case the hull received no damage whatsoever and had there been 6 to 9 inches more propeller clearance probably the machine would have made a clean take off and suffered no damage at all.

Communicated.—In amplification of my remarks during the discussion I should like to add the following:—

The time has arrived when the question of seaworthiness can well be divided into two classes: (*a*) At anchor or when taxying on the open sea; (*b*) Taking off and alighting.

These two cases present entirely different problems but are often confused when seaworthiness is being discussed. As regards the former the flying boat may be considered as seaworthy as any other surface craft of its size and it is towards improving the latter that there remains in my opinion so much to be done.

Taking the analogy of the largest surface vessels, the seaworthiness of the Berengaria for instance is reduced as the wind and sea increase and a reduction in speed is necessary to avoid damage. We must also find a means of reducing the speed of take off, or in effect time of take off if the flying boat is to escape damage. From an examination of this subject in its many aspects it looks as if we may find in the end something on the lines of the autogyro the only solution.

Mr. R. J. Mitchell: The flying boat has always been considered less efficient than the aeroplane. This view has now been proved to be erroneous, and one of the most interesting parts of the paper has been the figures and diagrams indicating this. Even with regard to durability, the flying boat is at least as efficient as the aeroplane, as it can now operate for something like 400 flying hours without requiring overhaul and reconditioning.

The great value of the flying boat rests with its ability to operate independently as a unit and to carry out long cruises away from its base. In order to do this, it must have a long range and be well equipped internally so that its crew can live on board. Cruises of this nature have already been carried out with great success, and have proved that flying boats can operate to a fixed time-table, almost entirely independent of the weather. It would appear that the ideal range to be aimed at for a British Service flying boat is about 1700 miles when carrying full military load, and this range is now well within reach.

There is one very important development of the flying boat which has not been stressed. This relates to the position in which the fuel is carried. Until fairly

recently it has been the general practice to carry the fuel inside the hull, but some of the latest designs carry fuel in the upper wings. This development has very many advantages. It very greatly adds to the comfort of the crew, as the presence of fuel in the hull is a very objectionable feature and is dangerous from the point of view of fire, and it gives the crew much more room for their operational work It was at first thought that removing the fuel from the hull to the upper wings would increase the structure weight; but it is found that if it is very carefully positioned so as to balance air loads, it can be made to help the structure and so reduce its weight. This, however, only seems to apply to a quantity of fuel about equivalent to 800 mile range. If larger quantities of fuel are carried the extra weight begins to have an important effect on the structure and increases its weight, due to the large fore and aft inertia forces brought into play. For long ranges it therefore appears that a new position will have to be found for some of the fuel in order to keep an economical structure weight. This position will probably be behind the engines or inside the lower plane.

With regard to the corrosion of duralumin hulls, I consider that the lecturer's statement that a duralumin hull can only be expected to last for fifteen months without showing serious signs of corrosion is incorrect. I understand that a very large proportion of the corrosion troubles which have been experienced is entirely due to the use of mild steel in contact with the duralumin. This definitely sets up corrosion. This difficulty can be completely overcome by the use of stainless steel for all fittings placed in contact with the duralumin, and if this precaution is taken a very much longer life can be obtained.

Mr. BAKER: Some of the pictures which Major Penny showed, took me back to the beginning of our experiments with these machines. I think that what he has said about the improvements that have recently been made, does not give the complete picture—I am speaking from the passenger's point of view rather than that of the pilot or Air Force officer. I took my first flight in a Sopwith seaplane in 1913. It was not comfortable. One never knew when one was really off the water, and when one came down it was often into a bath. I went up in one of the first F.B.A. machines in 1915. These were fearfully tail heavy and the pilot when landing had to incline it to an angle of 45 degrees. The hull was practically flat-bottomed and very broad and often broke. The only comforting thing was that if it gave in the pilot got the worst of it.

I am very pleased to see that Major Penny and I have reached agreement regarding the future of the flying boat. Three years ago when he expressed the official view of the position of the flying boat for the next ten years, his opinion of its advance was a long way behind my own. In his paper to-night he shows that he has gone a long way towards the view which I held at that time. I hope it means that the Air Ministry thinks the same.

I think that England will find that boat machines are necessary for its air commerce. It has been a false move to develop the land machine, which is more useful to other countries whose main routes will lie over continents, while our Empire routes must be over the sea.

In his paper Major Penny speaks of porpoising as being "obscure," I suggest the word "difficult" as more proper. The cause of porpoising lies in the fact that the machine when supported on two points necessarily "hunts," and the difficulty is to so arrange the rear step that the suction between the two steps is comparable with the upward force on the rear step.

In the paper there is a statement that the boat should *naturally* trim back at high speeds to the angle for take off, and I should be glad to know if this is the opinion of pilots in general. To my mind it is very undesirable, and

most modern machines are so arranged that at high speeds they trim forward to small angles, but can be eased back under control, and we have regarded that as correct design. We used to think that a machine should be trimmed back so that the pilot could put his hands in his lap and let it fly itself. This was actually done by us on the C.E.1 machine in 1916. The fact that this trimming back plan has not been copied shows that the official mind was against it.

With regard to metal hull construction, in naval construction you are mainly concerned with very large longitudinal forces and very small shearing forces. With aircraft design the shearing forces are the more important or equally important. The greatest variation of gravity I have met with on board a ship hull is two thirds gravity, but on flying boats I have seen trustworthy records up to 2.G. due to violent impact. It follows that the shearing forces can be very heavy and you must meet them. This is one of the weak points about flying boat construction at present. I think too that the idea of constructing metal hulls with large transverse and longitudinal stringers, with comparatively large unsupported areas between, will be found bad practice, and these areas will fail under local strain.

Lastly, I would like to make a few comments on the films that we were shown. The film of the model was not obtained from the National Physical Laboratory. I think it must have been done at Shorts'. When full-scale tests have been made, of hulls which had been tested in model form in the tank, reports would sometimes come in saying that the machine differed absolutely from the model, but on examination of the facts it was often shown that the agreement was really quite good. Pilots are the best observers but it needs a different type of man to interpret their observation. When we send our own assistants from the National Physical Laboratory we have found that the model and full scale data agree, and the R.A.F. officers have always concurred in the results obtained.

I know of only one thing of which it can be said that model results do not represent large scale results, and that is porpoising. Model tests will tell you when porpoising will begin but not what will happen when it has begun.

Mr. W. O. Manning: There is one point in connection with the duralumin hull which has not been brought out. In changing over from wood to duralumin you are changing from a brittle material to a ductile one, and the advantages are sometimes considerable. Much damage to wooden hulls has occurred on beach trolleys in the past. I feel certain that in many cases when wooden hulls have been pierced that duralumin hulls would have only suffered denting.

Group Captain Cave-Browne-Cave remarked on the beaching of seaplanes. In the case of a wooden hull, if there happened to be stones on the beach there is very little doubt that the stones would go through the hull, but would only make dents in a duralumin hull. A large number of dents would of course be serious, but a few would not matter.

Another point: Metal is a material that it is possible to join satisfactorily. Although wooden boats have been used for thousands of years no satisfactory joint has been found. With duralumin it is simple, and one feels confident of being able to make the hull an engineering job.

Corrosion: I feel pretty certain that a large amount of the corrosion found so far has been due to things that further experience with the materials would eliminate. Slight modifications in the alloy used might also effect an improvement.

I should also like to know if any experience has been obtained with Y alloy in salt water and whether the corrosion is greater or less than with duralumin.

Captain D. Nicolson: Flying boats have been greatly neglected in the past, but I hope that after such a large audience have heard the able lecture on a most interesting subject, that much more development will soon be carried out.

Major Penny let us see several slides and a film showing the spray caused by the hull pushing its way through the water. In all these flying boats it is noted they had wing root stay tubes, and if these were dispensed with, there would be very much less spray.

In every large flying boat the wing root stay tubes come down to the chine, and are below the L.W.L. when the hull is fully loaded, and are always a nuisance, as well as causing a good deal of broken water to be thrown up.

If stub wing root tubes were fitted, this would be a great improvement, but in metal hulls it is my opinion there is no need to fit them.

All the types of bottoms of hulls shown by Major Penny cause spray to be thrown out, and I am sorry he did not illustrate some of the types which run perfectly clean. The inverted vee-bottom hull is an example which runs cleanly, and does not throw out any water. In this type of boat, air is collected in the bows which helps to cushion the boat, and also reduce skin friction.

I had the pleasure of giving a lecture in this hall a few weeks ago on high speed motor boats, and during the discussion many eminent naval architects pointed out that the inverted vee-bottom type proved to be exceptionally clean running. My firm have built a number of inverted vee-bottom boats, and the latter have proved not only to be clean, but have a very good lateral stability. It is all a question of the amount of vee given, and the size of the channel (if any) at the top of vee. There are two or three other types of hulls which so far have not been tried on flying boats, but throw out no spray. We have tested a few models in tanks and also towed them behind fast launches, and had films taken of these, and the results were highly satisfactory.

We have had a good deal of discussion on seaworthiness, this is another point which has interested me for many years. I do not think we have built in this country a flying boat with a multi-step bottom, but high-speed motor boats with multi-steps are much more seaworthy than a boat with a single or double step; owing to the existence of several steps the boat is more easily kept in trim. Consequently, balancing or the position of the longitudinal C.G. is of less importance, and this feature is unquestionably the cause of the superior sea-going qualities of the multi-step boat, as it logically follows that a boat supported on several points will be steadier in a seaway than one supported on only two.

We have had many years' experience in designing and building high speed motor boats of all types, and have come to the conclusion that the multi-step is the best seagoing boat. At the present time, we are building a number of boats to do 50 m.p.h., and all these have multi-steps to make them not only racing boats, but cruisers, so that they can operate cleanly in any weather.

With regard to the subject of beaching flying boats, this is very important, and here the multi-step boat has an additional advantage, as the hull can be built complete, and the steps detachable, so that if one gets punctured it can be replaced by a spare nearly as quickly as changing a car tyre.

Several of our very light boats have stood up well for over 15 years, and have been beached regularly. If the boats have to encounter all kinds of shingle, a keel faced with greenheart is of great value.

I realise it is impossible for Major Penny to mention all types of construction in one paper, but I should have liked him to mention "Consuta" construction. Consuta makes an ideal job, as all plank seams are eliminated, there being only the chine and keel seams to make and keep watertight, which is a comparatively simple operation. Furthermore, there is a considerable reduction in labour, and "Consuta" construction lends itself to mass production as a flying boat hull can be built in five pieces. An example of this is the "Medina" flying boat hull.

I am not against metal construction for flying boat hulls, but there is difficulty in working the present duralumin, and as it corrodes very easily, it does not make the ideal material. If the boat were big enough, I think steel

would be much more suitable. There is a fruitful field for the discovery and application of materials, which, while possessing the essential characteristics of steels of the ordinary types, are not subject to corrosion. Further, the various practical considerations, such as machinability and adaptability to manufacturing operations, must be taken into account.

Major A. R. Low : He had put forward from time to time statements of the theory of structural weights of aeroplanes. The basis of all sound theories was the rigorously accurate theorem in mechanics that if geometrical similarity was maintained in the details the structural weight would increase as the cube and the surface as the square of the linear dimensions.

The next step was to lighten the webs, flanges and walls of the spars, tubes, etc., in inverse proportion to the increase in linear dimensions. A second theorem was now very accurately true (within a few per cent.) that the structural weight and the surface had both increased as the square of the linear increase, while the factor of safety had come down in inverse ratio of the linear increase. As an example, the U.S.A. had built 3-ton seaplanes with a factor of safety of 6, and a similar 12-ton seaplane—the N.C. type which crossed the Atlantic—with twice the linear dimensions, four times the surface, structural and total weights, and a factor of safety of 3.

These two theorems were not open to any serious question. But now a point of controversy arose. Many designers had stated that they could employ finer methods on the detail work of the larger machine. This was not a question of a mechanical theorem, but of the best constructional practice at a given time, and the way he dealt with it was to invite constructors to state what percentage of structural weight they could really save on, say, a fifteen-tonner as compared with a five-tonner. He had the considered statement of one of our ablest constructors that it was more than five per cent., less than ten per cent., and might be taken in conservative figures at about seven per cent. Seven per cent. was an important saving which would put up the limits of the type by about 14 per cent., but it was certainly no justification for attempts to design very large machines without limit. It was much simpler to keep this "fining down" factor entirely separate, whereas most writers tried to embody it in their theory by means of empirical formulæ which were useless without the particular sets of data on which the formulæ were based.

Lighter engines, better consumption, better aerodynamical forms, all tend to put up the limits each in its own well defined proportion, and he estimated the progress since 1915 as corresponding to an increase of the limiting size, from five tons to seven and a half tons; beyond some such figure the performance or factor of safety would be sacrificed unduly. He did not mean to say that it was desirable to go to these sizes. Generally speaking, the smaller an aircraft the better was its performance until the pilot's weight became a serious proportion of the total weight. But up to the effective limit of the day the useful load could be increased with the total weight, rapidly at first, more and more slowly as the limit was approached.*

He had applied these methods to the Army trials data of 1912, and had found that a five-tonner was the limit of useful size with an implied performance of about 5 hours flight at full load, 3,000 metres ceiling and nearly 600 h.p. and a factor of safety of six—unheard of figures for those early days. The distribution of weights was, power plant and fuel 40 per cent., structure 40 per cent., crew and useful load 20 per cent.

* In Alexandre Sée's book "Les lois expérimentales de l'aviation" (1910) a number of theories of aeroplane dimensions are reviewed with great acuteness. Plausible theories indicating unlimited possibilities of size are exposed, and the method sketched above is substantially adopted. It had always surprised him that Sée in 1910 should have so much clearer insight into the problem than most subsequent writers.

Within a few years Mr. Handley Page had produced a successful five-tonner with the invaluable co-operation of the Admiralty Air Department designers, among whom he would mention only Professor (then Mr.) Pippard. He considered this a great triumph of British design. Not content with this, Mr. Handley Page had obtained the support of the Air Ministry in producing his large 14-tonner, against the opinion of most of the design staff. He had notes of conversations with Mr. Handley Page and later with Professor Pippard, and scarcely needed to refer to these in stating that while, as a *tour de force* of design, it was an even more remarkable achievement than the five-tonner, it had failed to beat the laws of nature, on which the theory of limits was firmly based, and had sacrificed too much merely to getting a large total weight off the ground.

It was a favourite device of those who refused to accept the limitations of size, to make partial statements which might superficially seem to set the results of the theory in contradiction with facts. For instance, there was no need to tie oneself rigidly to the ratio 40 : 40 : 20 in power plant, structure and useful load. These figures could be varied within fairly wide limits in a large variety of ways. But he was content to refer such superficial critics to a study of Alexandre Sée's chapter on the size of aeroplanes. His argument was so well documented that he could quote only a few cases—the N 1000, the Porte Baby, the Tarrant Bomber, the Capronissimo, the Martin Bomber, all had tried to beat the laws of nature, and all had suffered defeat and sometimes disaster. He was content to let the exponents of unlimited size enjoy verbal triumphs, but they must also bear the responsibility of their costly material failures.

He had really been considering only large aeroplanes, but he was of the opinion that precisely the same methods would set the limits to the useful size of flying boats. The most obvious difference was that in purely reconnaissance flying boats the useful load could be cut down far below 20 per cent., with corresponding readjustment of the limits. But the limits were there just the same, and, he thought, the performance of a seaplane or flying boat must always be inferior to that of a land machine.

He thanked the lecturer for his paper, which made solid and extensive contributions to the progress of design.

Mr. HANDLEY PAGE : I heard with some astonishment Major Low credit me with a statement, which so far as I know, I never made in regard to the 15-ton 4-engine machine made by my Company during the war.

Major Penny in his paper and in the slides, which he has shown on the screen here to-night, has dealt with the question of relative size and the advantages and disadvantages which one gets from the large flying boat, and has made a comparison between the flying boat and the big land machine. I think that whilst he kept to the historical side of seaplanes and showed the slides in relation to them everything went well, but when he came to his figures and tables of comparison between the large seaplane and the large flying boat he seemed to go all wrong. If one refers to the diagrams and table in this paper it will be seen that the flying boat had a structure weight which has now, after ten years of development, been reduced to about 30.2 per cent. This is after ten years of hard development work in flying boat structural design. The machine of which Major Low spoke was produced in 1918 and built in wood, the total weight being 30,000 lbs., but the structure weight was only about 30 per cent. I call particular attention to the fact that this was a machine made in 1918 and built in wood. If Major Penny had put that machine on his diagrams and, with the keen imagination that all those at the Air Ministry possess, had proceeded to show what would happen in 1927 if there had been an equal development in its design I think we should have arrived at a structure weight for it of about 28 per cent. That actual machine, as Major Low well knows, had

a factor of safety of 4, and has flown with an overload of some 10 per cent. with perfect success.

I believe that the argument which Major Penny seeks to substantiate in favour of the flying boat in comparison with the land machine is difficult to uphold. The flying boat can have a much heavier loading and higher landing speed owing to the greater space that is available in harbours for landing flying boats and the absence of obstructions such as trees, etc., which often surround an aerodrome. If Major Penny had made his comparison on the basis of equal landing speeds the structure weight comparison would have been very different.

Then, too, the length of the flying boat is, as a rule, much shorter than that of land planes, which again makes for lighter construction, but I cannot believe that the shorter flying boat can have such good controllability as the land machine with its longer fuselage. As the flying boat gets bigger the designer can compress things into the hull and get less resistance and also cut down structural weight. If in land machines we reorganise our ideas as the machines become bigger and perhaps make our landing gear in an integral part of the fuselage we should get an improvement at least equal to the flying boat of the same size.

Captain COURTNEY: With respect to my visit to the Dornier works, I have only one point to make and that is this, that in the whole of the works no one has even seen, or knows anything about, wooden construction for flying boats. In this country the building of flying boats in metal has really only just begun. I think if Shorts had not done what they did with metal construction we should not be as far as we are now. The Germans do not claim to be better engineers than we are, though they rightly claim to have a very much larger experience than we have in this form of construction.

I would not care to join in any discussion as to whether duralumin or wood is the right material for flying boats. The Germans have far greater experience in the metal construction of flying boats than we, and they are going to push ahead with all their power.

Major BUMPUS: I am one of those who during the war were chiefly concerned with seaplanes and flying boats, and who felt keenly the relative neglect of such craft.

It is noticeable that in opening the discussion Group Captain Cave-Browne-Cave and Wing Commander Maycock both centred their remarks round seaworthiness as the crux of the problem of seaplane development.

Seaworthiness is a comprehensive term and presents a series of problems to the seaplane designer over and above those involved in the design of a corresponding aeroplane, the chief of which are those connected with getting the machine off the water. The necessity for ample water clearance for the propellers has been dwelt upon, but there are other factors of primary importance which must be carefully borne in mind.

I would suggest that the term seaworthiness is a misnomer as applied to the conditions obtaining between hump speed and flying speed, for no pilot of experience would willingly keep a machine running continuously at this part of the speed range in any but quite calm water. It may be argued therefore that the most seaworthy craft is the one which gets away with the shortest possible demonstration of this kind of seaworthiness, both as regards time and distance. This point of view is in accordance with war time experience, which indicated that a power loading of about 13lbs. per h.p., in conjunction with a stalling speed of 50 knots, was about the limit to permit of routine operation in all but actual storm conditions. More heavily loaded machines can, doubtless, be taken off safely by specially skilled pilots and by average pilots in chosen weather; moreover progress should be expected in the intrinsic merits of design, but to ensure adequate seaworthiness for general service conditions, and particularly for com-

mercial development, the get-away should not be inferior to that implied by the above figures.

Broadly speaking a high power loading is an indication of poor acceleration and, for a quick take off, necessitates a low stalling speed, and conversely a high stalling speed necessitates a low-power loading.

The present day requirements for increased load, and more particularly for long range, imply an increase in power loading, while the demand for high top speed immediately suggests a reduction of wing area with a consequent increase in stalling speed.

It must be borne in mind, therefore, that a simultaneous increase in these two factors implies a definite diminution of seaworthiness, the fundamental importance of which has been emphasised by several speakers.

Wing Commander T. R. CAVE-BROWNE-CAVE: That part of the lecture which deals with the history of these craft is, I think, the best historical summary that has been given us. I hope it will be possible to reproduce all the slides illustrating this section in the Journal when the lecture is published.

I am particularly glad that the question of seaworthiness has been dealt with so fully. It appears not even yet to have been established whether by increasing size seaworthiness is also increased.

Mr. Mitchell referred to the practice of putting petrol tanks on the wings. When I first became acquainted with flying boats it did strike me that the presence of petrol tanks in the hull—often well mixed up with wireless gear—was a great source of danger. The number of fires from this dangerous arrangement seems, miraculously, to have been small, but I hope that the necessity for more fuel will not mean placing even a small amount of petrol in the hull.

Mr. H. M. YEATMAN: The point has been raised of the difference of the state of the surface of the water in the model and full scale tests. It appears to me to be due to the fact that though you have similar conditions as regards the principal quantities determining the motion of the water, you have not got similiarity in regard to surface tension, and that makes the difference of water being broken up into spray in the full scale. I do not think it has any effect on the quantitative interpretation of the experiments.

With regard to the inverted vee-bottom boat, one speaker has said that the inverted vee-bottom is a very clean running boat, but can he say that the drag is as low as in the ordinary vee-bottom type? Messrs. Thorneycroft published some figures which showed that the horsepower required for this type of boat was absolutely prohibitive, and for their purpose the ordinary vee type had to be used.

I was very interested to see the film of the boat taxying on the water, as I was the observer in the boat (the Vickers Valentia) when the film was taken and I had not hitherto seen the result.

These figures referring to the tank experiments for the design of the first coastal motor boats were published in *Engineering*.

Mr. John H. NARBETH, C.B., C.B.E., M.V.O., late Assistant Director of Naval Construction, Admiralty: Major Penny has given us a very useful historical summary which definitely fixes certain points of considerable interest, and which I am glad to see makes honourable mention of the names of Lieut. Porte and of Mr. Linton Hope, both of whom in their lifetime did work of very great value.

The real importance of the paper, however, lies in the simple record of the successful treatment of so many difficult problems, and the final development of serviceable flying boats constructed with metal hulls. The Air Ministry is to be heartily congratulated that such splendid results have been reached.

I fully concur with the remarks as to the great future for flying boats.

Many people have been very greatly dissatisfied at what seemed the neglect of the flying boat in favour of the aeroplane by all British authorities. They will be pleased to hear that so many difficulties have been overcome, and they will hope that the publication of this paper is a sign that by the active encouragement of the Air Ministry flying boat construction has now reached such a practical position that a strong policy of advance in the provision of flying boats of considerable size both for naval and commercial services can be safely undertaken.

Colonel the MASTER OF SEMPILL: I cannot ask Major Penny to reply this evening, as time does not permit. I should, however, like to ask him one or two questions.

(1) What information has he as to the behaviour of the Dornier Wal under arduous service conditions?

(2) What is the total weight that this machine has, so far as he knows, flown at, and what was the loading per square inch and per h.p.?

(3) What is Major Penny's opinion as to the future of monoplane flying boats with hulls similar to the Dornier, but not necessarily tandem engines?

(4) Does Major Penny's experience of auroleumin, treated by the Anodeen process, show that corrosion is avoided where gradual corrosion due to landing, etc., occurs.

(Owing to the absence of Major Penny abroad, it has not been possible for him to reply to the discussion. The Appendices follow copy and have not been checked by Major Penny.—EDITOR.)

ROYAL AERONAUTICAL SOCIETY

AMALGAMATION OF THE INSTITUTION OF AERONAUTICAL ENGINEERS WITH THE ROYAL AERONAUTICAL SOCIETY

Statement of the Chairman of the Council, Colonel the Master of Sempill

I have been asked to prepare a brief statement of the progress of negotiations between the Royal Aeronautical Society and the Institution of Aeronautical Engineers, since the publication of the Beharrell Report in the March issue of the Journal.

On March 29th, 1927, the Sixty-Second Annual General Meeting of members of the Society was held. At that meeting the following resolution was proposed by Mr. H. T. Tizard and seconded by Mr. J. D. North:—

"That this meeting supports the action of the Council in endeavouring to come to an agreement with the Institution of Aeronautical Engineers on the lines discussed in the Beharrell Report, and authorises the Council, if satisfied that the general body of the voters of the Society and of the Institution of Aeronautical Engineers endorses their views, to carry the amalgamation into effect with such alteration to the draft terms of the agreement that appear to them desirable."

The chief grounds of criticism appeared to be that the Beharrell Report called for no scrutiny of the qualifications of members of the Institution of Aeronautical Engineers, who would have the right to become Fellows or Associate Fellows by merely signifying their intention to do so. The Council felt, therefore, that it was necessary to obtain the guidance of those members who had been unable to come to the Annual General Meeting. Every voting member was circularised with a copy of the Beharrell Report containing a statement on each clause showing the effect of the Report if it were put into operation, and each voting member was asked to state on a postcard whether he was in favour of the Beharrell Report or not, and to state his objections or any modifications he thought should be embodied to the draft terms. The Council decided, following the custom of other important bodies and societies, that the result of any such voting should not be considered decisive, one way or the other, unless a clear two-thirds majority of those voting was shown.

The voting was exceedingly close. Six hundred and fifty-five postcards were sent out and three hundred and seventy-four received, the replies showing one hundred and ninety-eight in favour of amalgamation and one hundred and seventy-six against amalgamation. The necessary majority to carry the terms was one hundred and twenty-four, against the actual twenty-two obtained, so the Council felt that they could not proceed on the lines of the Beharrell Report.

Many members wrote and pointed out that the crux of the whole matter was the absence of a scrutiny.

A letter was written by myself to Mr. Molesworth, Chairman of the Institution of Aeronautical Engineers, explaining the position. As a result of the letter a further meeting between representatives of the two bodies was arranged.

The meeting was an extremely friendly one, the representatives of both bodies realised the utmost importance of coming to some basis which would enable the amalgamation to become an accomplished fact and so enable both bodies to go ahead as one united body concerned with the "advancement of the Science of Aeronautics and its applications."

The Minutes of the meeting are as follows:—

JOINT MEETING OF THE REPRESENTATIVES OF THE INSTITUTION OF AERONAUTICAL ENGINEERS AND THE ROYAL AERONAUTICAL SOCIETY.

30th June, 1927.

PRESENT:

In the Chair, Air Vice-Marshal Sir Vyell Vyvyan.

Representatives of the Royal Aeronautical Society:	*Representatives of the Institution of Aeronautical Engineers:*
Captain P. D. Acland.	Lieut.-Col. Moore-Brabazon.
Mr. Griffith Brewer.	Mr. M. Bramson.
Major A. R. Low.	Mr. Howard-Flanders.
Mr. F. Handley Page.	Mr. N. J. Hulbert (Secretary).
Mr. J. Laurence Pritchard (Secretary).	Captain Lamplugh.
Col. the Master of Sempill (Chairman).	Squadron Leader Reid.
	Mr. Wingfield.

It was proposed:—

(1) That the Beharrell Report, with certain modifications, should be adopted.

(2) That Clause II. of the Beharrell Report should be amended to read as follows:—

In the Amalgamated Society the title Hons. Members of the Institution of Aeronautical Engineers shall be dropped. Hons. Members and Members of the Institution of Aeronautical Engineers shall continue to describe themselves as Members of the Institution of Aeronautical Engineers; and Associate Members of the Institution of Aeronautical Engineers shall continue to describe themselves as Associate Members of the Institution of Aeronautical Engineers. No person becoming a member of the Amalgamated Society in pursuance to these resolutions shall be liable to an entrance fee, and no such person shall be liable to an increased subscription during 1927.

(3) That the Joint Rules Committee shall become a Rules and Grading Committee, and be strengthened by the addition of two members each from the Royal Aeronautical Society and the Institution of Aeronautical Engineers, and that this Committee shall scrutinise the qualification of any member of either body desiring an additional grade in the amalgamated body.

(4) That the name of the combined body should be: "The Royal Aeronautical Society" with which is incorporated "The Institution of Aeronautical Engineers."

(5) That both bodies shall take immediate steps to confer with their respective Councils and—in case of the Institution of Aeronautical Engineers, with their Members—on the above proposals.

At subsequent Council Meetings of both bodies these minutes were discussed and it was unanimously recommended that the Amalgamation should be proceeded with on the terms of the Beharrell Report as amended by the meeting of June 30th, 1927.

The terms of the resolution of the Annual General Meeting of the Royal Aeronautical Society stated *inter alia* ". . . to carry the Amalgamation into effect *with such alteration to the draft terms of the agreement that appear to them desirable.*"

As the terms now arranged are merely such alterations, and as they conform with the generally expressed opinions of members of the Society, as revealed by the postcard vote, the Council are taking the necessary steps to put the Amalgamation into effect.

The terms of the resolution passed by the members of the Institution of Aeronautical Engineers at their Annual General Meeting did not admit of any modification of the Beharrell Report and, following the unanimous decision of their Council, a circular notice was sent to all members of the Institution from which the following is extracted.

"Your Council are bound by the decision of the General Meeting only to Amalgamate on the strict line of the Beharrell Report. In view of the fact that the terms now proposed are not less favourable to members than those previously agreed, your Council have unanimously decided in favour of the modifications, and propose to proceed with amalgamation on these lines, unless notice of objection is received from not less than twenty members prior to July 26th, in which case a General Meeting would be convened by your Council in pursuance of Rule 18 (c)."

A General Meeting of the Institution was called for on September 2nd, 1927, and at that meeting the proposals for amalgamation on the terms of the Beharrell Report, as modified, were carried by a large majority.

The main effect of the new terms of amalgamation is that every member of both bodies will have equal rights and privileges. There will be two new grades in the Royal Aeronautical Society, namely "Member of the Institution of Aeronautical Engineers" (M.I.Ae.E.) and "Associate Member of the Institution of Aeronautical Engineers" (A.M.I.Ae.E.). Any member of the Royal Aeronautical Society will have the right to join one of the grades providing the Scrutinising Committee is satisfied as to his qualifications and any member of the Institution of Aeronautical Engineers shall be entitled to call himself a Fellow or Associate Fellow of the Royal Aeronautical Society if his qualifications satisfy the Scrutinising Committee.

The Council feel that Fellowship and Associate Fellowship of the Royal Aeronautical Society, and Membership and Associate Membership of the Institution of Aeronautical Engineers, represent two distinct but equally useful grades in the aeronautical profession and are satisfied that the retention of both grades will serve a useful purpose.

The Rules and Scrutinising Committee is now considering the alteration of

such rules as may be necessary and when such alterations have been agreed upon they will be submitted to the general body of members. It will be the final step to make the amalgamation complete and to bring to a happy and fruitful ending negotiations which have been in active progress for some four years.

W. SEMPILL,

Chairman, Royal Aeronautical Society.

POLAR EXPLORATION AND SURVEY BY AIR

BY R. SCOTT HALL, M.SC., D.I.C.

Paper read before the Students' Section, May 5th, 1927.

One of the latest branches of scientific research in which aircraft have been employed is that of polar survey and exploration. It is well known how much time can be saved, and how much labour avoided by using aircraft for surveying large areas of difficult country. There is probably no part of the world more difficult of access than the regions surrounding the two Poles, and there is certainly no part of the earth where exploration and survey is harder to carry out.

For years all travelling in these latitudes has been done on foot with limited help from dogs. In the last few years the air has offered another means. The older method is one involving months of toil and privation, calling for the highest endurance and yielding very little in return. The new method involves perhaps more uncertainties, but the return is so great from the geographical and general scientific points of view that it is worthy of very serious consideration, and the expeditions of the past few years have shown how great its possibilities may be.

It is not the object of this paper to discuss the practical use of expeditions to the polar regions from, say, a commercial point of view. Their objects are usually either the advancement of scientific knowledge, for which ideal Man has probably sacrificed as much or more than any other, or else the achievement of a definite goal. For those who would be strictly practical, however, there is commercial value in such work. There is the possibility of mineral discoveries; the Spitzbergen coalfields are an example. Far more important, however, is the study of meteorological conditions which have such great bearing on those prevailing in more temperate regions. Meteorological stations established on the South Polar Continent for example might make possible the foretelling of years of drought in Australia and certainly could forecast weather in such regions as the Southern Indian Ocean. The possibility of circumpolar air services in the northern regions has yet to be investigated.

Advantages and Disadvantages of the Use of Aircraft in Polar Regions

The two great advantages of aircraft in polar work are, firstly, the high speed of transport from point to point as compared with sledging, 100 m.p.h. as against a dozen or so miles a day; and secondly, the speed and ease with which mapping and surveying can be carried out in combination with a comparatively small amount of ground co-operation. This last statement requires some qualification which will be dealt with under the disadvantages.

The disadvantages are formidable—this must be admitted at once. There is the danger of forced landings and also a far greater dependency on weather conditions, than is the case when sledging. It is impossible to examine the earth's surface in detail without landing—a great disadvantage from the geological point of view. Landings are very undesirable in these regions. Two-thirds of the surface are totally unfitted for a landing of any sort, and even if a safe landing be made, it may be days before it becomes possible to take off again.

There are several grave disadvantages of a technical nature which are not peculiar to polar work but apply in general to all air surveying on a large scale. The principal one is that to cover a large area a great number of exposures have

to be made involving very heavy labour in working up afterwards. Working at the usual height of about 4,000 to 5,000 feet, the scale of mapping works out at about 1 in 10,000, which is a very large scale for mapping in the ordinary sense, and involves quantities of unnecessary detail.

The question of dependency on weather and ground conditions brings us to the consideration of these, and also of the type of aircraft most suitable for use at the poles.

General Polar Conditions

Arctic

The north polar day extends roughly from mid-March to mid-September, during which time the sun reaches a maximum altitude of $23\frac{1}{2}°$. In Spitzbergen this means that there are ten to eleven days (of 24 hours) in the lunar cycle during winter on which there is complete darkness save for aurora and stars. If the weather is clear, radiation is unhindered, and winter ground temperature of $-40°$F. may be reached. If the radiation is hindered by clouds as it generally is, the summer temperature will probably stay round about 35°F. Temperatures at altitudes are sometimes higher than those at ground level.* Owing to the lowering of cloud strata, aviation in Spitzbergen for surveying purposes was only found possible during a few days of each summer month. Both Lieutenant Mittelholtzer and Mr. Binney, of the Oxford University Arctic Expedition, are unanimous on the scarcity of good weather for this purpose. These low cloud-banks are, however, often only local in extent. During fine days, winds are nearly always of less than 20 m.p.h. near the ground, and do not generally increase with height. Such winds as do occur, however, are very inconsistent in direction. The stillness and clearness of Arctic air under good conditions are ideal for aerial survey work. The air is not bumpy, and there are no differences of temperature between the day and night of the lower latitudes to produce vertical air current. Fog, unfortunately, is fairly frequent and occurs in layers of about 300 feet thick.

Both Commander Byrd, of the United States Navy, and Lieutenant Mittelholtzer emphasise the ideal conditions for flying to be met with in the Arctic in fine weather. The former gives an instance in illustration of a mountain which he could pick out at 150 miles.

Pack ice in the north polar basin is mostly unsuitable for landing owing to the heavy pressure ridges extending over the surface, frequent lanes of open water, and general irregularity. This applies, of course, to pack ice in the southern regions as well. Landing on the fjord ice of Spitzbergen is considered possible with ski runners, though not without risk of damage. The surface of glaciers, if snow-covered, provide safer landing with either skis or a flat-bottomed boat hull, and would be suitable for taking off also. The question of landing is most difficult, and the elimination of unnecessary alighting is essential. Commander Byrd had the utmost difficulty in getting his machine off from a specially-prepared snow track, and only succeeded after several attempts. The three-engined Fokker which he used was probably overloaded for its pole flight, however, and the skis which were used were also not strong enough in the first place. On the other side of the picture a Vickers flying boat has been landed successfully (and presumably taken off) from soft snow on a frozen lake near Murren in Switzerland.

Antarctic

The flying conditions of the Antarctic continent are to a great extent very much worse than those prevailing in the north. No work whatever has yet been

* See Appendix D for temperature gradient in Spitzbergen.

done with aircraft at the South Pole, though a flight was projected by Captain Wilkins for the purpose of surveying the coastline of Graham Land and King Edward VII. Land. He suggested using a Dornier Wal machine with suitable ground bases, but the flight has not been proceeded with to the author's knowledge, though originally planned for 1926.

Previously to this, Sir Ernest Shackleton had arranged to take a small aircraft with him to the Antarctic, but owing to a change of plan the machine was finally left behind.

Day and night or summer and winter are of course reversed here as compared with the North Pole. Midwinter is June 21st and Midsummer December 21st.

Sir Douglas Mawson, who led the Australian Antarctic Expedition of 1911-1914, a contemporary expedition to those under Scott and Amundsen, gives some interesting data on wind velocities in Adelie Land.

Average wind velocities for the three autumn months are:—March, 49 m.p.h.; April, 51.5; May, 60.7.

On August 16th, 1913, he records a gale of 105 m.p.h. for an hour.

Short spells of calm weather during the three months round the summer solstice are all that can be expected in this part of the Antarctic.

Adelie Land is, however, exceptional, and though blizzards are to be met with at all parts of the southern continent, at no other point probably do they reach such violence or continue for such long periods without abating. It is of interest to note that Mawson took with him in 1911 an "air tractor sledge," an aeroplane fuselage of primitive type with ski and wheel undercarriage, and fitted with a rotary engine. Another point to which he draws attention has special aeronautical interest. He gives the wing loading per square foot for the two commonest birds of these regions—the petrel 3.5 lbs. per sq. ft. and the albatross 2.4. It is interesting to note that these loadings are high in comparison with those common in birds of more temperate regions. It is, however, obscure how far south these birds penetrate.

The journals and reports of the British Antarctic Expedition of 1910-1913 throw further light on south polar conditions. These seem more hopeful from a flying point of view than those of Adelie Land. They are representative of conditions on the Ross Barrier and South Victoria Land. Blizzards occur but far less frequently than in Adelie Land, and are also less violent. Thus a gale reaching 84 m.p.h. for a short period during July, 1911, is spoken of as a record. The cause of these hurricanes appears to be the unstable conditions produced by the region of low pressure occurring over the sea due to the higher temperature of the air there as compared with that over the land. A point of great importance to air work is the suddenness with which blizzards arise.

The temperatures recorded by Sir Douglas Mawson are generally higher than those given by Captain Scott. The latter gives as his minimum record a temperature of $-77°$F. (109° of frost). In general the south polar summer is 15°F. colder than the north polar, giving a mean temperature of about 21°F. The south polar winter is 3° warmer than the north polar, giving a temperature of $-17°$F. This does not apply to regions such as the Great Barrier, where temperatures of $-50°$ and $-60°$F. are fairly frequent in the winter months.

The results of observation balloon ascents near the Ross Sea show an inversion of temperature for a few hundred feet and then a gradual fall,* so that the surface temperature is not reached again for 2,000 or 3,000 feet.

The ground conditions experienced by the British expedition of 1911 are not on the whole unfavourable for aircraft operation. Over much of the ground

* See also Appendix D.

ponies were used for sledging, and it appears that a smooth hard surface of frozen snow prevailed in many places. Soft snow was also encountered, notably on the lower slopes of the Beardmore Glacier.

Crevasses are a great source of danger on the glaciers when sledging, and would certainly be so to landing aircraft. Often they are uncovered, but more frequently there is a layer of snow across the top, rendering them infinitely more treacherous.

The regularity of the ice floes on the Ross Sea coast is much broken by pressure ridges and bergs. It is interesting to note that Captain Scott was of the opinion that wheels might be far more serviceable than the normal runners on the floe ice.

The great size and number of large tabular bergs in the Antarctic Sea is well known. Many of them are as much as $\frac{3}{4}$-mile by $\frac{1}{2}$-mile in area, though one 21 miles long has been recorded. They are sheer walled and often flat-topped, the surface being dead level or slightly undulating. Such bergs might be suitable for landing if not too badly fissured—a common fault.

The best weather conditions experienced in the Antarctic were those met with by Amundsen at his base at Framheim, and also over his route to the pole. He encountered very few blizzards, and the Framheim records show that calms were frequent—the wind velocity average being very low indeed.* On the other hand the temperatures he experienced were very low, $-13.4°$F. being his mean figure. There is, however, no doubt that the most ideal conditions for the operation of aircraft in the Antarctic are to be met with in this neighbourhood.

Types of Aircraft Suitable and Requirements for Polar Work

It may be said at once that the airship in any form is more or less unsuitable for prolonged work in either the Arctic or Antarctic regions. For a long single flight, such as Amundsen's voyage across the North Pole in the Norge, it is perhaps more suitable in some respects than the aeroplane, but for more scientific purposes its disadvantages are obvious. The rigid ship is ruled out on account of the difficulties of handling and mooring, and the dangers of weathering storms without suitable masts. In the Antarctic blizzard an airship, unless deflated and secured, would be wrecked at once. Indeed, the only type worth considering in either polar region is the small semi-rigid which has more structural strength than the non-rigid, and at the same time can be deflated very quickly. The airship has other disadvantages. The difficulty of gas supplies, its slow cruising speed, and vast bulk on the surface of which large quantities of snow might collect in flight, a danger which has already made itself felt.

The airship's advantages are two. It is far better for observation purposes, and it is easier to navigate in fog. This last point is of special importance in the Arctic.

The heavier-than-air craft is not nearly so handicapped as the airship. It is much smaller. It can be packed easily in a ship and transported in this way to its base.

A seaplane or flying boat might be flown from the sea or from ice or snow. For operation from the land or from pack ice a normal land machine fitted with skis might be the most serviceable.

The aeroplane has the great advantage of high speed in flight, and ease and quickness of handling on the ground. Its wings can be folded quickly and the machine made secure.

Its disadvantages are its unsuitability for observation purposes, the difficulty of navigation in fog, and its relatively small carrying capacity. For the purposes of survey work the last two are relatively unimportant. From the point of view of polar flights of long duration they are of distinct importance.

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* See also Appendix D.

Investigating the problems of the heavier-than-air machine further :—

For safety the aeroplane should be of the three-engine type to provide against forced landings, and an ample margin of power for getting off and climbing quickly should be given. The two outboard engines should be as near the fuselage as possible, and if even a slight means of access from the cabin could be provided in case of extreme need, so much the better.

The question of water-cooled or air-cooled engines for work in the Arctic is still in dispute. Both types have good records. The Rolls-Royce Eagles on Amundsen's Dornier Wal and the B.M.W. engine on Lieutenant Mittelholtzer's Junkers aeroplane in Spitzbergen uphold the laurels of the water-cooled; the Siddeley Lynx of the Oxford University machine, and the Bristol Jupiters used by the Swedish Army represent the air-cooled. The air-cooled engine would require suitable cowling, and this being provided the type appears a more satisfactory proposition than the other. In the Antarctic with its lower temperatures this type would probably be the only feasible one. Starting from the cold is the main difficulty, and preheating the mixture might be provided for.

Petrol-driven sledges were used by the 1911 Antarctic Expedition from this country. The carburettors were jacketed by exhaust gases, and although trouble was experienced in starting, the motors appeared to work fairly well subsequently.

Weather conditions and ease of maintenance point to a monoplane with an all-metal structure and covering. One of the best all-round records for Arctic work may be said to be held by the famous four-seater Junkers, a low wing cantilever monoplane with metal covering to wings and body.

The question of folding or quickly dismantling the wings beyond the two outboard engines should receive special attention.

Perhaps the most important point of all is the landing gear, and this involves a consideration of the form of body to be used. There are three possible schemes.

1. A flying boat with metal hull.
2. A seaplane with metal floats.
3. A machine fitted with skis.

The last of these recommends itself specially for the Antarctic, and for flying in the early part of the year in the Arctic. In both these cases it is reasonable to assume a fairly hard snow surface.

From consideration of general utility and strength, however, a metal hull seems the most practicable of the three schemes. It should be of extremely robust design and should have a flat bottom of not too great a surface area. Wing-tip floats or skis would also be essential.

Aerodynamically, the machine should have a low landing speed, say, 45 m.p.h., and a good control right down to this speed. High top speed is not essential, and 95 to 100 m.p.h. should be adequate.

For surveying and exploration purposes the machine should be equipped for a crew of three, the pilot and two observers.

All cockpits and cabins should be totally enclosed, at the same time good range of view in all directions for both pilot and observers being given.

The machine should be fully equipped with wireless (arranged so that it could be used when on the ground), navigation and surveying instruments.

Petrol and oil for a range of 500 miles at least should be carried. Ample provisions and sledging equipment for the crew would make up the remainder of the useful load.

For a polar flight from some convenient base the needs enumerated above would require considerable modification. In order to give the high carrying capacity needed for fuel, some light form of undercarriage fitted with skis would be most suitable.

The crew would be reduced to two—pilot and navigator. Instruments would be reduced to the minimum required for navigation of the machine. A wireless

set for emergency use and provisions would make up the rest of the load. The aeroplane would probably be heavily overloaded at the start in any case.

Of aeroplanes in actual existence, those which seem to recommend themselves most for easy conversion to polar needs are the following :—

Dornier "Wal" flying boat—twin Eagle engines.
Junkers G.24 L. seaplane—three Junkers L2 engines (230 h.p.).
Fokker F.VII aeroplane—three Wright Whirlwind air-cooled engines.
De Havilland 50 seaplane—Jaguar (Siddeley).

Of these machines, the Fokker F.VII as used by Commander Byrd on his North Pole flight seems to hold the greatest promise, with the "Wal" a close second. All four machines would require considerable modification especially as regards undercarriage before they could be used to their best advantage.

It is not within the scope of this paper to discuss the means of transporting the aircraft to its base in the polar regions. It is worth mentioning, however, that the vessels most suited to work in pack ice are small heavily-built whalers. It would be quite impossible to operate aircraft from the decks of such ships as they would be far too small and it would be unsafe to take any vessel large enough for deck landing to these latitudes.

Bases for Operations

In such a region as Spitzbergen for instance it might be possible to find many bays fairly free of ice during the summer months, and the aircraft for coastal surveying work could be operated entirely from the water. On the other hand there appears to be uncertainty as to the condition of these bays. Commander Byrd found King's Bay full of broken ice when he arrived for his flight in May, 1926, and freedom from ice cannot be relied upon. A locality is needed where the machine can be beached easily and where good natural shelter is provided.

In the Antarctic open water is far more uncertain and occurs for a much shorter period. It appears that McMurdo Sound is clear of sea ice from the end of January, roughly, to half-way through April, but how far this applies to other bays along the coast it is not easy to say. A sheltered land base seems the most suitable for these regions.

It is evident that flying can only be carried out during a short period of time in the summer months of the two poles. In the north the months of April, May and June offer the best conditions, and at the South Pole, December, January and February. If more than one aircraft could be taken, so much the better, since the work has to be condensed into as short a space of time as possible, and full opportunity taken of all fine weather.

The most important work for aircraft in the Antarctic is the mapping and survey of coastline, ranges of mountains and the like. This can be done without the necessity of elaborate co-operation from the ground and at very great speed. In Germany, aerial cameras are being developed to make the aircraft independent of ground triangulation. These work automatically and take both vertical and oblique views. The Goerz Optische Anstalt is largely concerned with this development.

Rough sketch mapping is of extreme value and can be carried out very readily from an aeroplane. The importance of the general bird's-eye view is very great. The lay-out of an area can be seen in a few minutes without the weeks of labour necessary in doing the same work from the ground.

These remarks are not, of course, restricted to survey work in polar regions, but apply to all difficult country and surveying on a large scale. The difficulties of aerial survey work are of necessity in attendance, with one or two additional

troubles peculiar to this work. These are mostly of a purely technical nature and are dealt with in Appendix B on polar air photography notes.

Geological survey work can also be carried out from the air, though this must be supplemented by detailed ground work.

Polar Flights

So far the problems of aircraft operation in polar regions have been considered almost entirely from the surveying and scientific aspects. A word should be said about straight flights to the poles. The problems here involved are two—firstly the carriage of sufficient fuel and oil, and secondly the navigation.

The first problem summarised is this :—For a North Pole flight from, say, King's Bay, Spitzbergen, sufficient fuel is required to give some margin over and above that required for 1,500 miles. Commander Byrd on his three-engined Fokker carried about 600 gallons, and used it a rate of 32 gallons an hour.

For a South Pole flight from McMurdo Sound, a distance of 750 miles, the same quantity of petrol should suffice. This gives a 23 per cent. margin for these flights.

The navigational problems are serious, and even more difficult is the definite location of the pole. It is hardly necessary to say that the magnetic compass is useless in these latitudes. Gyro-compasses are also useless. Astronomical observations of the sun in polar regions are not sufficiently accurate to rely on for keeping a course. The speed of flight is too great to obtain a good fix from successive observations, since the sun does not have time to turn through a sufficient angle between readings.

The most favoured methods of polar navigation up to the present are by the use of sun compass* in combination with drift observations of the ice pack or land beneath, and secondly by direction-finding wireless. The first method which has been employed successfully on several occasions suffers from the disadvantage that it relies on the absence of cloud above and fog below, though Commander Byrd suggests a method of navigating above fog with no view of the ground by using the difference of readings of a sun and a magnetic compass to give the drift. The second method suffers from the distortion effects well known in wireless directional work. These would probably be enhanced by the great distance from the ground station, and in the Antarctic the method appears impossible.

For the greatest safety, provision should be made for navigation by all three methods :—

(*a*) *Dead reckoning;* requiring airspeed indicator, altimeter, sun and magnetic compasses, drift indicator.*

(*b*) *Astronomical observations.* Instruments required—bubble sextant (*i.e.*, with artificial horizon), Bygrave slide rule for quick reduction of observation.

(*c*) *Wireless direction-finding.* Required—search coil and accessories.

Of all these three methods, two of which are essential to safe navigation, method (*c*) probably involves the heaviest equipment and is the least reliable in these parts of the world. This, therefore, would be the first to be discarded should the saving of weight demand it.

For a definite proof that the pole has been reached it is practically essential that a landing be made, and astronomical observations made at such a time interval as to give a good fix. If the observations taken are of the sun, as they almost certainly will be, this will involve a stay of several hours, and introduces an additional risk since polar weather changes rapidly.

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* See Appendix C.

* See note "Drift Indicator."

Summary of Polar Work

It is perhaps fitting to include a summary of the most important aerial work which has been carried out in polar regions. All of it is confined to the north polar basin.

In 1923 some interesting survey and mapping work was carried out by Lieutenant Mittelholtzer on a Junkers seaplane of standard type with slight modifications. He used King's Bay, Spitzbergen, as a base, and besides mapping a large area in Spitzbergen, took a number of very fine coastal and other photographs.

In 1924 the Oxford University Arctic Expedition took an Avro seaplane with Lynx engine to Spitzbergen for survey work. Much ill-luck was experienced with this machine, chiefly owing to insufficient time for trials previous to going north, and also to a serious engine breakage. Excellent work was done, however, and much experience gained.

In May and June, 1925, Captain Amundsen made his now famous dash to the North Pole with two Dornier Wal flying boats, all-metal monoplanes with Rolls-Royce Eagle engines. These machines were forced to land in latitude N.88° due to lack of petrol and a faulty engine. Successful landings were made with both machines in lanes of water in the ice pack. Great danger was experienced due to the closing up of the pack, and consequent risk of crushing in the sides of the hulls. N.24 was damaged and abandoned. N.25, after several hours of incessant labour, jumping on the ice wherever it threatened to pierce the hull, was raised intact on to the floe itself. Three weeks were then spent in preparing tracks from which to take off, and after many failures to rise the machine flew back to Spitzbergen successfully with both crews on board.

1924 and 1925 also saw survey expeditions from Soviet Russia to Nova Zembla. The pilot was Tschuchnovski, and a Junkers aeroplane was used. Many errors were found in the coastline of Nova Zembla as previously mapped, and a small new island was discovered.

In 1926 much Arctic aerial activity was shown. Commander Byrd reached the North Pole on a Fokker machine, with three Wright Whirlwind engines. King's Bay, Spitzbergen, was used as a base, and the whole flight lasted 16½ hours. King's Bay is about 750 miles from the Pole.

Captain Amundsen's flight in the semi-rigid Italian airship Norge across the polar regions from Spitzbergen to Teller in Alaska is too well known to need description. About 3,000 miles were covered in about 71 hours, and the pole was reached on May 12th. It was definitely established that there is no land whatever in the immediate neighbourhood of the North Pole.

Throughout the greater part of this flight the Norge was in wireless communication with the outside world.

Although the Wrangel Island Expedition employed no aircraft in its work, yet its object was the advancement of aviation. Wrangel Island has a valuable strategic position, should air routes ever become feasible in Arctic regions. It was with the idea of strengthening the British claim to the island that Stefanson sent out the expedition which was beset with singular misfortune, all the members save one perishing. This last was rescued by the second of two relief parties sent out.

The following passage occurs in the preface to Lieutenant Mittelholtzer's book *By Airplane towards the North Pole*:—"Henceforward airplanes will be indispensable to circumpolar exploration and their use will revolutionise this branch of scientific research."

APPENDIX A.

Equipment and Details of a Suitable Three-Engine Machine for Work in Polar Regions. (Based on figures for the Fokker F.VII. 3M.)

1. *Equipped for Survey and Geographical Work.*

	lbs.	lbs.
Total weight empty*		5000
Pilot and two observers	540	
Fuel for five hours (at full throttle)	1640	
Oil for five hours	160	
Instruments—navigation (including W/T but no D.F. gear)	120	
Instruments—surveying (aerial camera and cinema apparatus)	90	
Sledging equipment (three men)†	130	
Food (three men for one month)†	110	
Spares and tools for machine, etc.	20	
	——	2810
		7810

Wing area of F.VII. 3M = 630 sq. feet.
Wing loading of above machine = 12.4 lbs./sq. foot.
With 3 × 200 h.p. engines, power loading of above machine = 13.0 lbs. h.p.

Fokker Commercial F.VII. 3M. (3 × 200 h.p. Wright Whirlwind Engines).

	lbs.
Total weight empty	4750
Useful load total	3200
	7950

Wing loading, 12.6 lbs./sq. ft.
Power loading, 13.3 lbs./h.p.

The performance of the Fokker is:—

Landing speed, 47 m.p.h.
Maximum speed (ground level), 125 m.p.h.
Cruising speed, 106 m.p.h.
Climb to 1,000 feet in 1.2 mins.
Ceiling, 15,500 feet.

The performance of the survey machine will be very slightly better owing to the reduction of all-up weight.

Notes.

(*a*) A wireless installation is not included in the empty weight figure of 4,750 lbs. given for the normal Fokker F.VII.

(*b*) The sledging equipment includes sledges and harness, tent, sleeping bags, gun and ammunition, ski sticks, snow shoes and crampons for three men, stove and cooking utensils. See also *With Seaplane and Sledge in the Arctic*, Binney.

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* Margin of 250 lbs. left here for conversion of undercarriage, etc. C. F. Fokker, F.VII. 3M.
† These figures are based on those given for two men by G. Binney, *With Seaplane and Sledge in the Arctic*.

2. *Equipped for a Polar Flight.*

Distances:—North Pole from King's Bay, Spitzbergen, 750 miles.
South Pole from McMurdo Sound (Ross Sea Coast), 750 miles.

	lbs.	lbs.
Empty weight of machine		4750
Pilot and navigator	360	
Fuel for 18½ hours at cruising speed*	4500	
Oil for 18½ hours	590	
Navigating instruments†	100	
Food‡	40	
		5590
Total all-up starting weight ...		10340

For the normal Fokker machine, getting-off speed = 60 m.p.h. (say). Hence for machine equipped for pole flight as above the getting-off speed

$$= 60 \times (10{,}340/7{,}950)^{\frac{1}{2}} = 68.5 \text{ m.p.h.}$$

and wing loading = 16.4 lbs./sq. ft.; overloading = 30 per cent.

APPENDIX B.

Aerial Photography in Polar Regions

The calmness of the air in north polar regions during fine spells has been emphasised already. This is a great help towards steady flying and good photography.

The general requirements* for an aerial camera for polar work are the same as those for any survey work. They are:—

1. The camera should be automatic in the following respects:—Plate or film changing, shutter setting and making continuous exposures at fixed intervals for correct overlapping.

2. On each exposure should be recorded compass bearing, date and time, optical centre of photograph, altitude, and if possible the angle of tilt of the optical axis from the vertical (this would require a gyroscopic stabiliser).

The special polar requirements are connected with illumination. The illumination in these regions in fine weather during the polar day is very intense, and exposures should be as short as possible. To get a long shade scale the use of a yellow disc is necessary in polar work to cut out the blue rays. Lieutenant Mittelholtzer advises the use of a panchromatic plate and a developer rich in reducing agents.

APPENDIX C.

The Sun Compass (Goerz Type)

In this instrument the sun's image is reflected through a periscope on to a disc in front of the pilot. The periscope is driven by clockwork turning it one complete revolution during the mean time between the two consecutive passages of the sun across the upper meridian. The periscope can be set in any position relative to the fore and aft axis of the aircraft, and the position of initial setting is determined by the time at which the flight is started. If departure occurs at any time of day other than noon, the right ascension of the sun for the time at

* At cruising speed this gives a margin of 23 per cent.

† This includes a W/T set, sun compass and sextant, besides instruments normally fitted to machine and included in the empty weight.

‡ This gives some allowance in case of forced landing until relief could be sent.

* See paper read before the Institution of Aeronautical Engineers by F. S. Barton, M.A., *Air Photography Apparatus*, November 30th, 1926.

which the clock movement is to be released must be calculated, and the instrument adjusted accordingly. If the start be made at noon, the periscope points straight down tail.

The periscope can be adjusted for the sun's declination, and also for change of latitude since the axis of the periscope has to remain parallel to the earth's axis.

(See article in *L'Aeronautique*, March, 1926.)

APPENDIX D.

Meteorological Notes (Miscellaneous)

The following temperatures gradient for Spitzbergen July are given by Mr. Binney in the account of the Oxford University Arctic Expedition of 1924 *With Seaplane and Sledge in the Arctic.*

Ground, 35°F.
1,000ft., 37°.
2,000ft., 34°.
3,000ft., 29°.
3,800ft., 28°.

From observations at McMurdo Sound in the Antarctic by the British Expedition of 1910-13 it was found that at a height of 13,000 feet the temperature was only 9°F. below that at the surface.

The following figures are given in Dr. Simpson's meteorological summary, *Scott's Last Expedition*, Vol. II.

Temperature.

Cape Evans. 77° 35′ S.		Northern Station. 77° 30′ N.	
October	−4°	April	−5.4°
November	12.4°	May	15.4°
December	21.5°	June	33.1°
January	20.7°	July	36.9°
February	18.6°	August	34.2°

Frequency of Winds.

Wind velocity m.p.h.	Yarmouth. %	Framheim. %	Cape Evans. %
0 to 4	5.2	42.2	29.8
5 ,, 9	23.0	25.9	16.0
10 ,, 14	28.4	16.1	8.1
15 ,, 19	19.0	8.0	7.7
20 ,, 24	11.5	3.6	7.5
25 ,, 29	5.7	1.9	7.8
30 ,, 34	3.0	1.4	7.4
35 ,, 39	2.1	0.6	5.9
40 ,, 44	1.5	0.3	4.7
45 ,, 49	0.7	0	2.9
50 ,, 54	0	0	1.5
55 ,, 59	0	0	0.7
Greater than 60	0	0	0.2

Note on Drift Indicators.

The best form of drift indicator is the wind gauge bearing plate for tail observations. From the point of view of exposure and consequent frostbite, however, it is essential to have an instrument which does not necessitate the observer exposing himself to the airstream, and thus the aero bearing plate for vertical observations is most suitable as it can be used with a glass door in the floor of the cabin.